ANTHROPOLOGY AT THE BRITISH ASSOCIATION.

THE scientific carnival of Great Britain has again come and gone. The "wise week", as the good people of Newcastle called it, was on the whole most successful. The profuse hospitality of Sir William Armstrong, and the people of Newcastle generally, will long render this meeting memorable in the annals of the British Association. How far has the science of Anthropology been advanced by this meeting? All branches of science have their own accounts to render; and it is only our duty to examine the amount of advance made in the science of Mankind. Anthropology in name is not yet recognized in theory; but it is to some extent in practice. It is not a little remarkable, that some of those who are most opposed to the recognition of Anthropology as a recognized branch of science into the Association, are the very men who, in their practice at least, admit the claims of Anthropology, and who read papers which are entirely anthropological. For instance, Mr. Crawfurd, one of England's most consistent and venerable ethnographers, lost no opportunity of protesting against the introduction of anthropological papers into Section E; and yet, with that inconsistency for which he is occasionally distinguished, was one of the very first men in the section to read a paper on a purely anthropological subject. Mr. Crawfurd's paper, entitled "Notes on Sir C. Lyell's Antiquity of Man", was from the beginning to the end a paper on Man or Mankind, as distinguished from Ethnology, or the science of the Races of Man. No writer of any authority, either English, American, or continental, will now call the question of the antiquity of man an ethnological question. It is pure and simple an anthropological question. Other papers bearing on the same subject, we understand, were rejected by the Committee of Section E, because they were anthropological! and could not be read because Anthropology was not recognized by Section E, which was entirely confined to Geography and Ethnology. But such an example from one of the most eminent Vice-Presidents of the Section, could not fail to have its influence on other members; and the result was that, notwithstanding several anthropological papers were refused by the Committee, still there were a larger number accepted. An analysis of the papers read in Section E gives the following results. There were altogether forty-one papers an-

nounced to be read before the Section; eighteen of these were geographical, nine were ethnological, and fourteen anthropological. Besides, there were several anthropological papers, which were not accepted by the Committee simply because they *were* anthropological. Most of the geographical papers, we believe, were original; but there were only five out of nine of the ethnological papers which had not been read before, and, in the words of the President, "amply discussed" in London. All the anthropological papers read before Section E, including two which were not read, were, with one exception, never read or discussed before any other scientific body. It will, then, be seen that anthropologists have yet much work to do before their science can be generally recognized. But, as far as practice goes, they have cause for satisfaction; and the recognition of Anthropology in theory must soon follow its recognition in practice.

On the whole, therefore, we have no hesitation in saying that the general result of the meeting must be considered satisfactory to anthropologists. Several circumstances combined to make Section E one of the most popular sections, as, indeed, it always has been when at all properly conducted. In the first place, the Section was presided over by the prince of presidents, who was a host in himself, and who, we are bound to admit, contributed far more than any other man to make Section E popular and its proceedings satisfactory. Sir R. Murchison was free from the littlemindedness shewn by some of his associates. His whole conduct in the chair was both fair and honest; and all his exertions were used to render the meeting agreeable to all parties. Thus, we know he frequently felt it his duty to remain at his post to his own serious inconvenience. We can only regret that his other high duties, as one of the chief rulers of the Association, caused him to occasionally absent himself. There was no one at all capable of filling the post like Sir Roderick. It is no disparagement that his two countrymen, who acted occasionally in his absence, were far from being so successful in their presidency as their eminent friend. These statements are acknowledged truisms: but we are bound to say that even Sir Roderick occasionally failed to give satisfaction to all parties. We have received several letters complaining of "the apparent puff" which Sir Roderick appended to his introductory address! We certainly were ourselves a little surprised to hear Sir Roderick coolly enumerate a list of ethnological papers to be read before the Section, most of which, as we have stated, had been read long ago, and were "stale, flat, and unprofitable". Out of the six ethnological papers which Sir

Roderick announced to be read, there was only one which had not been read before! In a previous part of his address, he had mentioned some of these papers as having been "*read and amply discussed*". As to the strong remarks we have received respecting this part of the President's address, we would observe that in all cases they have been from those who were not personally present to witness the support and courtesy which Sir Roderick invariably gave to anthropological papers and to anthropologists. We would also observe, that Sir Roderick gave a *vivâ voce* statement, which was not printed, at the end of his address, in which he acknowledged the valuable additions of papers the Section was likely to have from the representatives of the Anthropological Society of London. Nor do we think that Sir Roderick meant to do more in what he said than give his aid to ethnological science. We feel sure that he cares far too much for truth to care for any one set of men more than another. We ourselves are grateful for any aid he renders to ethnological science. We are as much interested in the result of ethnological science as of general Anthropology. There may be difference of opinions as to the best means of advancing the Science of Mankind; but we are sure that there is no difference of opinion as to the importance of Ethnology, or the Science of Races. Nor do we think that any man is worthy of the name of an ethnologist, who looks with disfavour on those anthropologists who believe that the Science of Mankind embraces something more than Ethnology. Rather ought they to rejoice to see the great success which is attending the labours of their fellow-workers. The British Association is for the advancement of science, perfectly regardless of personal opinions or party cliques. We feel sure, therefore, that it only requires a little time to remove any jealousy that may exist in the breasts of some ethnologists respecting the success attending the labours of anthropologists. Let them learn not to quarrel with the decrees of Nature. Astronomy was not arrested in her progress by the clamours of the astrologers; nor will anthropologists cease to develope the extent, magnitude, and importance of their science by the invectives of ethnologists. Rather let them develope their own subject, and look with rejoicing on the beneficent wave which will ere long raise them from their present state of isolation, and raise them to their place as one of the branches of light which will illuminate the great system of organic life.

We will now give a general abstract of the anthropological papers read at the Association. On a future occasion, more of these papers will be printed at length. We have classed the papers under two

heads: General Anthropology;* and one special branch of that subject—Ethnology.

GENERAL ANTHROPOLOGY.

On Anthropological Classification. By Dr. JAMES HUNT, F.S.A., President of the Anthropological Society of London.—After the author had given a short outline of the nature of the subject, in which he distinctly stated that the origin of man belongs entirely to mythical times, and is a question which could not at present be solved by human experience, he proposed merely to classify man as he now exists, or as he has existed since the historical period, without reference to those distinctions being absolutely original. It was Dr. Hunt's duty to inquire—were these well-defined differences in mankind at the earliest dawn of history? a question which he answered in the affirmative, as the ethnology of the most anciently known continents is very much the same as at the present day. He considered also that these differences had been permanent; and the scope of the present paper was to inquire whether these physical differences were so well marked as to serve as the basis of classification. He reviewed the classifications of Ephorus of Cuma, Buffon, Linnæus, Gmelin, Herder, Voltaire, Blumenbach, Lacépède, Duméril, Maltebrun, Cuvier, Virey, Hunter, Lawrence, Metzan, Bory, Desmoulins, Prichard, Lesson, Fischer, Morton, Latham, Hombron, Jacquinot, D'Omalius D'Halloy, Pickering, Burke, Knox, Agassiz, Crawfurd, and Isidore Geoffroy St. Hilaire, and offered critical remarks on each of these systems as a whole. Many of them were of the most arbitrary nature, the offspring of chance or human fancy, unfounded on the knowledge of any ascertained facts, and there was no attempt to define the method on which a sound anthropological classification might be based. The multiplicity of the systems at present in vogue is a sufficient refutation of the truth of most of them. Dr. Hunt considered that anatomy and physiology were the primary sources whence an adequate knowledge of the principles of anthropological classification could be derived. Language he considered no test of race. He laid great stress upon the form of the cranium as the most convenient and certain distinctive mark, and spoke with great approval of the ternary classification adopted by Gratiolet, who divides mankind into the Frontal (European), Parietal (Mongol), and Occipital (Negro) races—these cranial distinctions being coincident with the mental and moral characters which were

* To those papers which were not read before Section E, we have affixed the letter of the Section after the title of the paper.

solely dependent on man's physical structure. Other secondary physical characters could also be used with advantage; and Dr. Hunt especially alluded to the classifications which might be based upon colour, stature, hair and beard, longevity, diseases, temperaments, odour, entozoa, and other subsidiary points of distinction. The degree of intelligence was the chief character distinguishing man from the inferior animals. If a classifier of the negroes of the West Indies were to use language alone as a criterion, he would classify them under the head of Europeans, with whom their acquired language is identical; their physical characters alone mark them as African. Dr. Hunt considered that language must be utterly discarded as the first principle of anthropological classification. He gave a far higher value to religion, and to art, considering language merely as the third element. It was possible to change the language of a race; but apparently impossible to change either their religion or their innate ideas of art. That there are well-marked physical, mental, and moral distinctions in mankind is as well an ascertained fact as that there are differences in the orang and the chimpanzee. We must, therefore, classify mankind according to the physical, and psychological differences which now exist, for the present state of anthropology will not enable us to say how and when these distinctions have originated.

Cranioscopy of South American Nations. By Mr. C. Carter Blake, F.G.S., F.A.S.L.—The object of the paper was to re-consider some of the primary principles on which those cranioscopists who have classified the races of South America, have based their arrangements, and to call especial attention to a few important exceptions which appear to invalidate the generalisations commonly accepted. Every practical cranioscopist is aware that Retzius's classification of human skulls into brachycephalic and dolichocephalic was applied by that illustrious Swede to the arrangement of the great leading South American types. The lamented and deceased cranioscopist gave, as as examples of the brachycephalic type, as exhibited in South America, the tribes of Ecuador, Peru, Bolivia, Chile, La Plata, Patagonia, and Tierra del Fuego; while the dolichocephalic or longheaded type found its representatives in the populations of Carib, Guarani, Brazilian, Paraguay, and Uruguay origin. This broad generalised statement of facts still remains the accepted and predominant hypothesis. How far is it consonant with the extent of our knowledge on the subject? Those few tribes and nations of South America of which any accurate and reliable information exists will be briefly recapitulated in the

following observations, and especial attention drawn to the *desiderata* which appear in our collections. The geographical order will be adhered to, apart from any broad generalisation, which may arise, based on craniometrical classification; such generalisations, *e.g.*, as that of Morton, who divided the whole American races into two great families, the Toltecan, comprising the extinct half-civilised tribes which have become extinct during a pre-historic period, and the barbarous tribes. The latter division was subordinated amongst the Appalachian, Brazilian, Patagonian, and Fuegian branches. Mr. Blake then proceeded to criticise these types in detail. In the first place, he pointed to Colombia; the characteristic type prevailing amongst the tribes of Venezuela is the Carib. The skull is here markedly long-headed, with the parietal diameter less than the longitudinal. The frontal bones are strongly flattened; the zygomatic arches large. Accurate and reliable evidence respecting the cranial conformation of the natives of Ecuador is wanting. The Cara and the Scyri unknown. There were several types in Peru; *e.g.* the Chincha type shortheaded; the Chimu type longheaded, so far as known; the Inca or Quichua shortheaded, flattened from before to behind by compression from the frontal bone to the occiput. In Bolivia there were the Aymará, longheaded, of which few examples existed in our collection; the Titicacan, longheaded, but of whom the other physical characters are unknown. In Chile the type was longheaded, so far as known at the present day. The Anthropological Society of Paris has recently sent a series of queries respecting the physical characters of the Chile races, which showed the utter want of information on this topic. In Patagonia the type was also longheaded, as in Tierra del Fuego, Paraguay, La Plata, and Brazil.

Commodore MAURY asked the author of the paper whether there was any relation between the distribution of any of the cranial types alluded to and the distribution of the inland basins. An attention to physical geography would, he was confident, throw much light on the question of race.

Mr. MARKHAM pointed out that the Quichua and Aymará tribes were distinct as regards language. The Aymará language was as distinct from the Quichua as the Italian from the Spanish. The Chinchas were far more nearly allied to the Quichuas than were the Aymarás.

Mr. CRAWFURD, after complimenting the author of the paper on the industry with which his materials had been collected, denied that cranioscopy afforded us any sound knowledge of the affinities of races. He would tell an anecdote which was entirely new to the British Association. Professor Owen on one occasion had described a skull

which was really that of a Scotchman, as that of a Negro. Therefore, Mr. Crawfurd concluded that knowledge of the cranium was no guide to the affinities of races. He complained that Mr. Blake had not offered any generalisation as to the number of indigenous stocks in South America, and stated that no such generalisation could be arrived at by mere cranioscopy.

After a few remarks from Mr. GREENFIELD,

Dr. JAMES HUNT said that he had intended not to offer any remarks on the interesting paper that had just been read, but he could not silently listen to the observations of Mr. Crawfurd without rising to protest to the British Association against the sneers which Mr. Crawfurd was in the habit of casting in the teeth of anthropologists who devoted themselves to the science of man. Section E. had become notorious for their neglect of all true science relating to man. All other sections made advance from year to year, but Section E. did not. The same subjects were discussed every year, and no progress was made or would be until anthropology became recognised by the Association. Many men of science devoted to this subject despaired of doing any good by attending the Association. Dr. Hincks said yesterday that Mr. Crawfurd was entirely ignorant of the science of language, and he was obliged also to say that his friend, Mr. Crawfurd, was not competent to judge of the value of cranioscopy as a basis for the classification of man. It was useless to argue with one who rejected both physical and physiological characters as a basis of classification, and one who was also opposed to the evidence of language.

Professor WILSON said he was sorry that it had been suggested that the subject under discussion was of no importance. He was not a craniologist, and therefore would not presume to offer an opinion upon the questions at issue, but he would mention a fact which had some bearing upon them. He was acquainted with a hatter in Canada, carrying on an extensive business both with the English and French communities. He took the measure of his customers' heads according to the Paris fashion, and he (Dr. Wilson) had collected the models—upwards of a hundred—and, with the assistance of a scientific friend, had classified them, without referring to the names affixed to them, in two distinct groups, the English and the French. Upon examining the names, it was found that, with two or three exceptions, they had made a perfectly right classification, though the only data they went upon was the shape of the skull. The science of craniology might have been carried too far, but he was sure it was calculated to lead to very valuable results. There were no doubt distinct national types of skulls, and he hoped that anthropologists, instead of being discountenanced would receive every encouragement from the British Association.

Mr. CARTER BLAKE, in reply, agreed with Captain Maury with respect to the advantages which were derived from a comparison of the cranial types with the geographical localities. He answered Mr. Crawfurd's complaint respecting the absence of any generalizations respecting the origin of the South American natives, by saying that he was quite content to wait and accumulate facts. As regards Mr. Crawfurd's amusing anecdotes, he was afraid his learned friend had put the

cart before the horse. Professor Owen did not mistake the skull of a Scotchman for that of a Negro; it was a Negro skull he mistook for that of a Scotchman. Physical characters alone could decide the affinities of a race; and as Mr. Crawfurd had rejected the test of language, he failed to perceive what the characters were on which his classification was founded.

Sir RODERICK MURCHISON, after commending the learning and ability of the paper, hoped that the science of anthropology, which had been founded by his friends, Blumenbach, Retzius, and Von Baer, would ere long be recognised by the scientific world.

On the Physical and Mental Characters of the Negro. By Dr. JAMES HUNT, President of the Anthropological Society of London.—The author said he had been collecting facts upon the subject for another society; but he was induced to bring it before the Association from the fact that it had never been brought before a scientific audience in England. In discussing the question, he would have nothing to do with anything but the full-blooded, woolly-headed, typical Negro, to the exclusion of the half-breed. The object of the paper was to determine the position which one well-defined race occupies in the genus *homo*, and the relation or analogy which the Negro race bears to animated nature generally. He had selected the Negro race, as it seemed to be an intermediate form between the highest and lowest existing races of man. In discussing the question, he had nothing to do with the origin of man, for analogies did not necessarily include relationship. The skin and hair are by no means the only things which distinguish the Negro from the European, even physically; and the difference is greater still mentally and morally. The skeleton of the Negro is generally heavier, and the bones are larger and thicker, in proportion to the muscles, than those of the European. The bones are also whiter, from the abundance of calcareous salts. The thorax is compressed; the leg is longer than in Europeans, but is made to look shorter on account of the ankle being only between $1\frac{1}{3}$ in. to $1\frac{1}{2}$ in. above the ground; the heel is both flat and long. Burmeister has pointed out the resemblance of the foot and the position of the toes of the Negro to that of the ape; and many observers have noticed that the Negroes have frequently used the great toe as a thumb. After pointing out several minor particulars, in which the Negro differs from the European, and quoting the opinions of several writers on the capacity of the Negro cranium, the paper recommended caution in accepting such capacity of the cranium as any absolute test of the intellectual power of any race. The brain of a Negro has a smoky tint, not found in that of an European. The hair is essenti-

ally different; and the voice resembles sometimes the alto of an eunuch—there being a peculiarity about it by which he can always be distinguished. Dr. Louis Büchner, after summing up the peculiarities of the Negro, says they exhibit the most decided approach to the ape. Other distinguished anatomists and physiologists had expressed a similar opinion. The assertion that the Negro only requires an opportunity for becoming civilized is disproved by history. The African race have had the benefit of the Egyptian, Carthaginian, and Roman civilization, but nowhere did they become civilized. The many cases of civilized blacks are not pure Negroes; but, in nearly every case where they had become men of mark, they had European blood in their veins. In the West Indian Islands it has frequently been observed that all the Negroes in places of trust which require intelligence have European features. Negro children are precocious; but no advance in education can be made after they arrive at the age of puberty —they still continue mentally children. It has been said that the present slave-holders of America no more think of rebellion amongst their full-blooded slaves than they do of rebellion amongst their cows and horses. That was because the tranquillity of Negoes in their approach to civilization resembled the content of domestic animals. From all the evidence brought forward, the writer of the paper saw no reason to believe that the pure Negro ever advances further in intellect than an intelligent European boy of fourteen years of age. After citing authorities to prove the low psychological character of the Negro, the paper continued:—"We now know it to be a patent fact that there are races existing which have no history, and that the Negro is one of these races. From the most remote antiquity, the Negro race seem to have been what they now are." The writer could see no evidence to support the opinion of some writers that the Negro had degenerated from some higher form of civilization. Everywhere we see the European as the conqueror and the dominant race; and no amount of education will ever alter the decrees of Nature's laws. The general deductions he would make were—First, that there is as good reason for classifying the Negro as a distinct species from the European as there is for making the ass a distinct species from the zebra; second, that the Negro is inferior intellectually to the European; third, that the analogies are far more numerous between the Negro and the ape, than between the European and ape. There was in the Negro that assemblage of evidence which would induce an unbiassed observer to make the European and Negro two distinct species.

Mr. GALTON said that the case was briefly this:—Among the Negroes

of Africa there were more frequent instances of an abject and superstitious character, combined with brutal behaviour, than could be paralleled elsewhere in the world. It was a wonder that people like those of Dahomey could mould themselves into any form of society at all, and it was actually found that when the chief of such a tribe died it disintegrated and rapidly disappeared. In short, the tribes of Africa were remarkable for their rapid formation and short continuance. Many of their chiefs were of alien descent, and it was remarkable how their greatest kingdoms had been ruled by Tawareks—men with Arab blood—or, as Captain Speke now informed us, by straight-haired Wahumas. How did it happen, then, that so degraded a people could furnish men capable of constructing nations out of the loosest materials? The question once stated was almost its own reply. The Negro, though on the average extremely base, was by no means a member of a race lying at a dead level. On the contrary, it had the capacity of frequently producing able men capable of taking an equal position with Europeans. The fact of a race being distinguished by the diversity of its members was well known to ethnologists. There were black and red sub-divisions of many North African races, and the contrast between the well-fed and ill-fed classes of the same tribe of Negroes was often such as to amount apparently to a specific difference.

Mr. CRAFT said that though he was not of pure African descent he was black enough to attempt to say a few words in reference to the paper which had just been read. Many scientific gentlemen present would probably dispute that; but at any rate, supposing Adam to have been the founder of a race of men, white men had no stronger claim to him as their father than black men, as it was admitted that owing to the climate in which he commenced his existence, he could have been neither black nor white, but copper coloured. As Africans were very dark, and the inhabitants of Northern Europe very fair, and as, moreover, the nations of Southern Europe were much darker than those of Northern Europe, it was perfectly fair to suppose that climate had a tendency to bleach as well as to blacken. The thickness of the skull of the Negro had been wisely arranged by Providence to defend the brain from the tropical climate in which he lived. If God had not given them thick skulls their brains would probably have become very much like those of many scientific gentlemen of the present day. The woolly hair was not considered by Africans as a mark of inferiority, though some of them shaved it off, but it also answered the purpose of defending the head from the sun. With regard to his not being a true African—his grandmother and grandfather were both of pure Negro blood. His grandfather was a chief of the West Coast; but, through the treachery of some white men, who doubtless thought themselves greatly his superiors, he was kidnapped and taken to America, where he (Mr. Craft) was born. He had recently been to Africa on a visit to the King of Dahomey. He found there considerable diversities even among the Africans themselves. Those of Sierra Leone had prominent, almost Jewish features. Their heels were quite as short, on the whole, as those of any other race, and upon the

whole they were well formed. Persons who had any knowledge of Africans knew that, when they enjoyed advantages, they were capable of making good use of them. He might refer to the instance of the little girl brought to this country by Captain Forbes. This child was presented to the Queen, who had her carefully educated. When she grew up, she mingled in good society, and interested every one by her proficiency in music, and recently she had been married to a commercial gentleman of colour at Lagos. Another case was mentioned by Mr. Chambers in one of his works, and another case was that of Mr. Crowther, who was well-known to many gentlemen in this country. One word with reference to the ancient Britons. When Julius Cæsar came to this country, he said of the natives that they were such stupid people that they were not fit to make slaves of in Rome. It had taken a long time to make Englishmen what they now were, and, therefore, it was not wonderful if the Negroes made slow progress in intellectual development. It was, however, proved that they made very rapid progress when placed in advantageous circumstances. As to the Negro not being erect, the same thing might be said of agricultural labourers in this country. He pointed to Hayti as furnishing an instance of independence of character and intellectual power on the part of the Negro, and contended that in America the degraded position which he was forced to occupy gave him no chance of proving what he really was capable of doing. He was sorry that scientific and learned men should waste their time in discussing a subject that could prove of no benefit to mankind. He spoke with great deference to their opinions, but, for his own part, firmly agreed with Cowper that—

> Fleecy locks and black complexion
> Cannot alter nature's claim;
> Skins may differ, but affection
> Dwells in white and black the same.

The Rev. H. B. Tristram said he had been a chaplain for several years in a mixed school for blacks and whites in one of the West Indian islands, and could testify that the children of free Negroes who were engaged in honourable occupations were invariably more intelligent than the children of slaves.

Mr. Carter Blake said that he agreed substantially with the author of the paper upon the anatomical evidence which he had brought forward with regard to the Negro race. Mr. Craft had stated that the heels of the Negro were not longer than those of Europeans, but that was contrary to the testimony of anatomists. He contended that in nearly every instance of a Negro attaining intellectual eminence it had been ascertained that there was an admixture of European blood in his veins. The evidence of the paternity of full-blooded African Negroes, in contact with Europeans, was rather difficult to ascertain. Anatomists had ascertained that there were wide differences in the structure of the Negro and European, and he specified some instances of such diversity. If the woolly hair and thick skull of the Negro were given to him by a bountiful Providence to fit him for living in a tropical climate, the inhabitants of Brazil were suffering great

injustice, for they had neither woolly hair nor thick skulls. With regard to the philanthropic element, he thought it ought not to have been introduced into the discussion. In conclusion, he expressed his opinion that, till Mr. Craft could rail away the seal which nature had impressed on the physical character of the Negro, his breath was all spent in vain when he contended for the equality of the African and European races.

Sir E. BELCHER said that he had spent nearly all his life among the Africans, and believed that, when properly educated, they could be as true, as faithful, and as sound-hearted as Englishmen. He mentioned several instances which he had met with in his travels of remarkable intelligence in the Negro, and scouted the idea that he was naturally, either physically or mentally, of inferior capacity to other human races.

Professor WILSON claimed for the author of the paper and those gentlemen who supported him, the credit of being influenced by a desire to search out facts which could throw a light on the important subject under discussion. At the same time he differed considerably from them in some of their conclusions. It was very important to have sufficient data before forming a theory, and he thought that Sir Charles Lyell and others who contended that the intellectual progress of the Negro stopped at the age of fourteen, had fallen into the error which a person would who went into a workhouse among the most degraded and wretched of its occupants to find intellectual culture and capacity. The fact was that very few, if any, black children had any opportunity of pursuing their education after the age of fourteen, and, in addition to that drawback, they had the misfortune to belong to a degraded and oppressed class, which was crushed and held down in the social world. The wonder was that any of these unfortunate people had energy enough to make their escape and to acquire knowledge sufficient to enable them to carve a way for themselves through life as not many of them did. The English character was made up of many elements; but a few hundred years ago the inhabitants of this island were cruel, unlettered, practical people. When put under good training and subjected to certain influences the Anglo-Saxon proved capable of remarkable development, owing chiefly to the native energy of his character. The same quality was observable in some tribes of the present day, such as the New Zealander, and it was not wise therefore to argue as though the absence of combination denoted a natural and primary inferiority of race.

Mr. CRAWFURD made a few observations on Negroes generally.

Dr. HUNT in reply said he was sorry that some speakers had attempted to draw away the attention of the audience from the great facts under discussion. Scientific physical facts had been met with vague general assertions, and no reply had been attempted to be made by any speaker against the facts that had been adduced. He would leave his scientific friends to judge of the value of Mr. Craft's remarks. He was sorry, however, that the speaker had not confined himself to uttering exploded theories, but had accused scientific men of wasting their time when discussing this subject. He for one

thought it was a great pity that scientific men in this country had so long delayed to bring these facts prominently before the public, and thus explode some of the popular delusions on the subject. It was not at all necessary for Mr. Craft to tell anyone at all acquainted with the subject that he was not a pure Negro, although there were many present who were deluded with the idea that he was. As to the statement that Britons did not make good slaves, he was quite ready to admit the fact; and he knew of no European race that would make good slaves. In this respect Negroes were certainly far superior to Europeans. He then briefly replied to other speakers, and in conclusion, said the time was passed when the great fact he had brought forward could be longer ignored, and however reluctant he had been to introduce the topic, he felt that good would arise from the discussion that had taken place. All he asked was that scientific evidence of this character should be met by scientific argument, and not by poetical clap-trap, or by gratuitous and worthless assumptions.

On Cranial Deformities, more especially on the Scaphocephalic Skull. By WILLIAM TURNER, ESQ. (D *a.*)—The Author commenced by stating that deformities of the skull might be occasioned by artificial means, by pathological changes, by posthumous changes, and by developmental irregularities and deficiencies. He in a great measure restricted himself in his paper to a consideration of the effects produced on cranial form by developmental irregularities and variations in the mode of ossific formation, more especially by premature or retarded union of the cranial bones along their sutural lines and at their synchondroses. He arranged the sutures connecting the bones of the skull cap into a vertical-transverse group, a median longitudinal, and two lateral longitudinal groups; and agreeing with Professor Virchow, of Berlin, he stated that should a premature ossification take place in one, or more than one, of the whole, or a part, of a line of sutures, then the growth of the skull corresponding to, and in a direction perpendicular to the line of synostosis will occur, and diminished length, or breadth, or height, as the case may be, will be occasioned. He illustrated this proposition by describing a peculiarly elongated and laterally compressed form of skull to which, along with Professor Von Baer, of St. Petersburg, he applied the name Scaphocephalus. Four as yet undescribed examples of this peculiar boat-shaped skull had come under his notice. The whole of these crania were characterised by possessing the following characters:—Absence of a sagittal suture, and consequent blending of the two parietal bones; absence of parietal eminences, lateral compression, great elongation. He then discussed at length the two theories which had been advanced to account for the production of such a form of skull;

and concluded that the balance of evidence was in favour of the theory that it originated from a premature union of the sagittal margins of the two parietal bones, and consequent compensatory growth of the skull in the antero-posterior direction, rather than from the development of the bi-parietal bone from a single median vertical centre. The author then directed attention to the importance of attending to the above proposition in ethnological inquiry, more especially with reference to the production, through its action, of various aberrant forms of skull in individuals of any given nationality, which may cause them to possess a shape of head quite different from that of the race to which they belong. He pointed out, moreover, that obliteration of the sutures to a greater or less extent exists in the crania of the Flathead Indians, which have been distorted by artificial means; his observations agreeing with those of Professor Daniel Wilson in this particular. He was of opinion that the pressure occasioned the tendency to premature union of the bones in these cases. The author did not think that persons possessing crania the form of which had been modified by premature synostosis necessarily exhibited any special tendencies to cerebral disease or deficiencies in their mental capacities.

Human Cranium from Amiens.—Mr. WILLIAM TURNER read a few notes, by Mr. Henry Duckworth, on the circumstances attending the discovery of a human skull at Amiens, and also a short paper of his own on the anatomical characters of the said cranium. The skull was produced and exhibited before the section. The cranium was found by Mr. Henry Duckworth, F.G.S., in the summer of 1861, whilst on a visit to the quarries of St. Acheul. It was dug out of the deposit named by the workmen the "Découvert" Bed. Its depth from the surface was about six feet. The anatomical description by Mr. Turner comprised an account of the appearance of the bones, and of the form and general characters of the skull. One of the most interesting points connected with it being its remarkable resemblance to the much discussed "Engis" skull, of which it might almost have been considered to have been a reduced copy. There was nothing in the appearance of the skull, or in the circumstances of its discovery to lead to the supposition that it threw any light on the question of the antiquity of man.

Mr. R. A. GODWIN-AUSTEN thought that the discoveries at Amiens had no bearing on the antiquity of man, as the whole of the locality had been a burying-place for an enormous period of time. He had visited the locality where the skeleton was dis-

covered, from which the famous jaw-bone, which had attracted so much attention, was taken; and he believed that the deposit there was nothing but an accumulation of drift from the chalk hills which overhung that particular spot.

The Neanderthal Skull. (C.) Professor WILLIAM KING gave reasons for believing it to belong to the Clydian period, and to be specifically distinct from man. He contended that the Neanderthal man was living in the concluding division of the glacial or Clydian period. He felt it necessary to advert to a question involved in the present subject, and on which a preconceived opinion, amounting to a prejudice, is pretty generally entertained. Some authors have no hesitation in admitting that the genius *Homo* has been represented by more species than one now living; but there is unquestionably prevailing a deep-rooted conviction that the psychical and speech endowments of *Homo sapiens* are generic; although there is nothing to warrant such a belief, and much to oppose it. He saw no reason to doubt that there have been species of the genus in existence, unpossessed of those gifts which so eminently place the existing human races, but in different degrees, above the highest anthropoid apes. Why may there not have been a Pliocene, or a Clydian species, possessed of no higher faculties than such as would enable it to erect a protecting shed, fashion a stone for special purposes, or store up food for winter; but like the gorilla, or chimpanzee, be devoid of speech, and equally as unconscious of the existence of a Godhead? Man's psychical endowments are visibly expressed in the prominent frontal and the elevated vertex of his cranium. But considering that the Neanderthal skull is eminently simial in its great characters, he felt himself constrained to believe that the thoughts and desires which once dwelt within it never soared beyond that of the brute. The Andamaner indisputably possesses the dimmest conception as to the existence of the Creator in the universe: his ideas on this subject, and on his own moral obligations place him very little above animals of marked sagacity, nevertheless they are such as to specifically identify him with *Homo sapiens*. Furthermore, the strictly human conformation of his brain-case bears out the collocation. Psychical gifts of a lower grade than those characterising the Andamaner cannot be conceived to exist: they stand next to brute benightedness. Applying these arguments to the Neanderthal skull, and considering its close resemblance to that of the chimpanzee, and, moreover knowing that the simial peculiarities are unimprovable—incapable of moral and theositic conceptions—he saw no reason to believe otherwise than that similar darkness

characterised the beings whom he did not hesitate to call *Homo Neanderthalensis.*

The Anatomy of a Young Chimpanzee. By Dr. EMBLETON (D *a*).—On the 11th December, 1862, the body of a male chimpanzee, said to be about one year and a half or two years old, and which had died of bronchopneumonia, in a menagerie, at Newcastle, was purchased for the College of Medicine. It was scantily covered with black hair, except around the muzzle and arms, where the hair was silvery grey. It was fresh and in good condition, the trunk rather bulky, the chest large, the arms strong and muscular, the hands partly covered on the dorsum of the palm with black hair, which did not extend to the fingers, the palm or surface smooth, naked, and of a dusky flesh colour, the thumb small and short, measuring with its metacarpal bone, 2 in., the middle finger being 5 in. long, the legs comparatively short and weak, but fleshy to the heels, the feet rather more covered on the dorsum with hair than the hand, the toes and the soles resembling in smoothness, absence of hair and colour, the corresponding parts of the hands, the great toe freely detached from the others, and resembling a strong thumb, measured with its metatarsal, $2\frac{1}{2}$ in., the third toe, $3\frac{1}{2}$ in. The thumb appeared much shorter, slenderer, and weaker than the other fingers; the great toe thicker, stronger, and shorter than the other toes. The following dimensions of parts were carefully taken:—Length from vertex to the sole of the heel, 2 ft. 5 in.; length from top of sternum to tuber ischii, 1 ft. $\frac{3}{4}$ in.; length of leg from top of femur to sole, $11\frac{1}{2}$ in.; length of arm, from head of humerus to tip of middle digit, 1 ft. 5 in.; length of hand and foot, each $5\frac{1}{2}$ in.; circumference of chest at broadest part, 1 ft. $4\frac{3}{4}$ in. The whole body weighed 16 lb. 6 oz. avoirdupois. Owing to its tender age, and the necessity for preserving it, the skeleton was not much studied, and time did not allow of dissection of much of the muscular system. It may be observed, however, that there exist thirteen pairs of ribs and, therefore, thirteen dorsal vertebræ, and in consequence the number of lumbar vertebræ is reduced to four. The diaphragm was well arched, and very strong; the psous parvus muscle was present, and attached as in a man. The skin, arranged by Mr. John Hancock, according to the exact dimensions and form of the animal, was deposited in the museum of the Natural History Society, and the skeleton, carefully prepared by Thomas Craster, in the collection of the College of Medicine. A dissection of the muscles and tendons of the palm of the hands is shown in sketch F. It was observed that the opponens pollicis muscle was

wanting; the others appear to be disposed as those of the human hand. Professor Huxley having maintained, in his *Man's Place in Nature*, that the hind limbs of the so-called quadrumane is not a hand, but in reality a foot, it was necessary to direct particular attention to the muscles and tendons of that part. Sketch C shows the posterior region of the leg, which is flat, and rather broad, and the fleshy parts of the lateral muscles are continued down to the ankles; the gastrocnemii are the principal features hiding the presence of the soleus, and the absence of a plantaris. Sketch B, fig. 1, presents the anterior region and the dorsum of the foot. The peroneus brevis, which is inserted into the fifth metatarsal, arises here above the peroneus longus, the tendon of which, passing behind the outer ancle, runs obliquely into the sole of the foot.

Next internal to the peronei lies the rather slender extensor longus digitorum, the four tendons of which pass to the four outer toes. Between this muscle and the edge of the tibia lie three muscles, one being a good deal overlapped by the other two. These two send their tendons to be inserted, the *inner* into the inner side and under part of the first cuneiform bone, the *outer* into the base of the metatarsal bone of the great toe. The third muscle, at first deeply placed, comes out, a little above the ankle, from beneath the other two, and its tendon, lying between that of the outer of the two and the tendon of the long extensor, runs to be inserted upon the dorsal surface of the base of the first phalanx of the great toe. On the dorsum of the foot we find the short extensor of the toes, a broader muscle, and extending further towards the inner side of the foot than in man, by means of considerable superadded slip, which diverges abruptly inwards from the other part of the muscle, sends its tendon along the metatarsal bone of the great toe parallel with the tendon of the last muscle, to be inserted into the base of the second or terminal phalanx of that toe.

Every toe, then, in the chimpanzee, has at least a long and a short extensor for its phalanges, whilst the great toe has an extensor for its metatarsal, another for its cuneiform bone. Thus it may be said that there are four muscles of the great toe to ensure free and varied mobility in the sense of extension; the fifth toe has, as in man, in addition to its phalangeal extensions, the peroneus brevis attached to its metatarsal. Of the four extensions of the great toe, the two innermost appear to represent the tibialis anticus of human anatomy, divided to secure variety of motions in the root of the great toe; the next would quite answer to the extensor proprius pollicis only; it is inserted

into the base of the first instead of the terminal phalanx; the fourth or short extensor is a new foot muscle, and unrepresented in either the hand or foot of man. Thus, in the peroneal region, and in that of the extensor, we find all the corresponding human muscles represented; moreover, there are certain modifications of arrangement, and a new muscle for the first digit introduced, to give more freedom and variety of movement in extension to that member. This muscle is a foot muscle, not a hand one; the divided tibialis anticus is rather a foot than a hand arrangement. There are no hand muscles introduced, though the great toe has four, and the thumb three extensors. There is here a great toe, more moveable in extension than any thumb.

We now turn to the sole of the foot. The three superficial muscles, the abductor pollicis, the flexor brevis digit., and the abductor minimi and digitorum are, as in the human sole, the first to come into view. On detaching the two last from the heel bone, we find, towards the outer border of the foot, a flexor brevis minimi digiti, and in the middle region the lumbricales and the tendons of the long and short flexors of the toes, with a small muscle accessory to the lumbricales arising from the long flexor tendon before its division. No muscular accessories arising from the os calcis and attached to the long flexor tendon were observed. At the outer border of the foot, when we abduct strongly the great toe, which can thus be brought to nearly a right angle with the rest of the foot, we see, after a little dissection, the abductor pollicis as a short doubly reuniform muscle, extending from the heel to the base of the first phalanx of the great toe, occupying considerable space, and close to it lie the two halves of the flexor brevis pollicis, separated by the tendon of the flexor longus pollicis. Lastly, between the great toe and the second is clearly to be observed the abductor pollicis. All these muscles of the great toe are highly developed and of great power; and, if they all act together, will very forcibly pull the great toe towards the middle of the sole of the foot; if the flexors of the other toes are made to act at the same time, the result will be a strong, rather oblique opposition of the great toe to the other four toes; and if an object like the branch of a tree be placed in the sole, it will be grasped with much firmness. There remains, however, to be noticed an interesting arrangement by which that action will be enforced and made more secure. It is this: the muscle called flexor longus pollicis is largely developed in the leg, extending down to the inner ankle, and ends in a strong tendon, which runs into the sole of the foot close to the os calcis, and apart, as in man, from the other tendons, opposite

to the foot of the great toe, it divides into two slips; one, the lesser, runs outwards at a certain angle, being confined at first under a strong ligament, as under a pulley, to the great toe, as its long flexor tendon; the other, the larger division of the tendon, passes straight onward to the other toes, supplying each with an additional tendon. It will be obvious to any one inspecting sketch E, that when this muscle (therein named flexor digitorum et pollicis) is put in action, it will necessarily draw the great toe and the other four toes together, and that simultaneously, towards the middle of the sole. This addition of a fourth set of flexor tendons for the four outer toes, and with it a distinct provision for the simultaneous action of those toes with the first, is very remarkable, and seems to complete the foot of the chimpanzee as a perfect instrument of prehension. The absence of the human flexor accessorius may be presumed to be an advantage, as it provides more space in the sole for the object grasped, and as no transversus pedis was found, the distal ends of the metatarsals are left more free to separate and enlarge the grasp. Next to the surface of the bones was beautifully seen the tendon of the peroneus longus muscle, resuming, in its bony and ligamentous groove to its destination, the base of the metatarsal bone of the great toe. When we consider, then, this elaborate mechanism, and see that the opening of the foot is most carefully provided for by the existence in connection with the four outer toes of a short and a long extensor for each, and by the presence of four distinct extensors for the great toe alone; and observe that flexion or prehension is made certain, exact, and powerful, by the arrangement of flexors for each of the four outer toes, vigorous short muscles of the great toe, and a necessarily synchronous action of the great toe and the other four, we cannot avoid the conclusion that we have examined the most admirable prehensile organ adapted to arboreal life that we can imagine; we must also feel persuaded that the hinder limb of the chimpanzee is still a foot, a prehensile foot of high perfection, surpassing even the hand itself in firmness and precision of grasp, but not a hand.

The additions to the ordinary mechanism of the human foot that have here been noticed, are not, so to speak, borrowed from the hand, but are either extensions of the plan of the foot, or new parts that occur neither in the hands nor in the foot of man.

The function of prehension by the foot, as is well known, is one enjoyed, not only by apes and monkeys, but by many other animals; the parrots, cockatoos, and other birds, and the chameleon, to cite familiar examples, have prehensile feet; it is attached to the nasal

organ in the tapirs and elephants, to the lips in the giraffes, horses, etc., and to the opposite or caudal end of the spine in certain monkeys and marsupials. Prehension, therefore, cannot be taken in the characteristic function of the hand of the higher animals.

Digestive Organs. The tongue, broad, fleshy, soft, and delicate, much resembled the tongue of a child. The milk-teeth, twenty in number, somewhat blackened, were all present. The total length of the alimentary canal was 15 feet 10 inches, or about six and a half times the length of the body. It is thus made up, viz.—

	Ft.	Ins.
From lip to pharynx	0	3
,, pharynx to cardia	0	6
Length of stomach along greater curve	1	$8\frac{1}{2}$
,, duodenum	0	6
,, jejunum and ilium	10	0
,, caput cæcum coli	0	$2\frac{1}{4}$
,, appendix vermiformis	0	$4\frac{1}{2}$
,, colon, ascending, transverse, & descending	1	$4\frac{3}{4}$
,, sigmoid curve	0	7
,, rectum	0	4
	15	10

Outlines of the stomach and cæcum accompany the paper. The œsophagus is somewhat narrow; the stomach is shorter and more globular than in man; the left end, or *cul de sac*, well defined; the pyloric extremity, funnel-shaped and abruptly bent back towards the cardia, which it nearly touches, is slightly marked off at the bend by a constriction, and there are two other smaller constrictions between this part and the duodenum. The drawing of the caput cæcum coli does not require particular notice. The peritoneum appears to be disposed very much on the human model, the foramen of Winslow, for instance, and the bag behind the stomach, were quite human.

The liver, with the gall-bladder, all the parts at the transverse fissure being cut close off, weighed $10\frac{1}{2}$ ozs. Its vessels and membranes resembled those of a child. It is divided into two great lobes, right and left, and each of these has a small rather detached lobule situated behind the transverse fissure, and bordering on the fissure of the vena cava. The spleen is rather thin, longish, and notched on its anterior border. The suprarenal glands are long and of a yellow colour, contrasting with the kidneys, which are brown, and also unlobulated.

Skull and Brain. The vault of the cranium having been removed, casts in plaster were at once taken by Mr. John Hancock of the inner surface of the bone of the brain covered by the dura mater, and, after

the brain was removed, of the cerebral surface of the base of the skull. Thus a correct cast of the whole cerebral surface was secured. Before the brain was in any way disturbed from its natural position, the relation of the posterior lobes of the cerebrum to the cerebellum was carefully observed; and the ten persons present, anatomists and naturalists, were unanimous in declaring that the former projected backwards over the latter a quarter of an inch. In the removal of the brain, the disposition of the membrane and nerves was observed to be strikingly similar to the corresponding human parts. The arterial circle of Willis was quite human. The entire encephalon with arachnoid and pia mater, vessels and nerves attached, and as much of the spinal cord as could well be reached by an ordinary scalpel, was carefully removed, and its weight was found to be 13 ozs. and 6 drachms, which is to the weight of the whole body nearly as 1 is to 19. The vessels having been removed, and the membranes, the whole brain was put at once into spirits for preservation and hardening, so that the separate weights of cerebrum and cerebellum were not taken. The three great lobes of each cerebral hemisphere were seen well developed; the two anterior lobes formed together a blunt projection forwards, whilst beneath their inner borders projected as ridges downwards, the under surface of these lobes were distinctly concave; the middle lobes were more prominent downwards than in man, and the projection of the posterior lobes backwards, overlapping the cerebellum, appeared as decided as before. After the brain had been for three days in spirits, the cerebral hemispheres measured in length $4\frac{3}{8}$ ins., in breadth across the middle or widest part $3\frac{3}{4}$ ins., the greatest width of the cerebellar hemispheres being $2\frac{7}{8}$ ins.; so that the cerebellum is markedly overlapped laterally as well as posteriorly by the posterior lobes of the cerebrum. The convolutions of the cerebral hemispheres were numerous, somewhat intricate, and partially symmetrical, two main sulci, traceable one from the Sylvian fissure, the other from the base at the back of the crura cerebri, appeared to mark out even on the top of the hemispheres the division into anterior, middle, and posterior lobes, or masses of convolutions. The island of Reil in the fissure of Sylvius was quite evident with three small convolutions. The corpus callosum, $1\frac{3}{4}$ in. in the length, showed as in man distinct though minute transverse striations, and a longitudinal raphe formed of two slightly raised lines and an intervening groove. A section of the right hemisphere, to expose the lateral ventricle, showed as bold and as numerous projections of the external sulci into the white centrum ovale as are commonly seen in

the human cerebrum. The ventricle itself was beautifully distinct, its anterior cornu curving boldly outwards in front of the striated body, its middle cornu winding outwards and downwards to the very bottom of the large middle lobe, and containing the hippocampus major and the corpus fimbriatus, and the choroid plexus and its posterior cornu extending in an ample curve backwards and inwards, so as almost to touch the grey matter of the surface next the median line, and having within it the projection called hippocampus minor, which may still be seen. On the floor of the body of the ventricle are to be seen the corpora striata, the tæniæ semicirculares and the free edge of the fornix with the choroid plexus; these last lie on the velum interpositum, which covers the third ventricle and the optic thalami, quite as in man. Further investigation in this direction was forborne, as it was thought desirable to preserve, for the present occasion, the parts already enumerated. The fourth ventricle, as it is called, and its walls, as they could be examined without injuring those parts, were inspected; the cavity was closed behind, and had its small choroid plexus after the human pattern; a second similar, but smaller, plexus existed on each side, just outside of the ventricle, and attached to the cerebellum. The point of the calamus scriptorius was well defined, but no white lines of origin of the auditory nerve were distinguished; on the other side of the medulla oblongata, the usual nerves were met with, and the pyramids and olivary bodies clearly to be seen, but they were not further examined. The cerebellum was laminated, and had the great human divisions; on examining that part which overhangs the medulla oblongata, the inferior vermiform process, the uvula and tonsils, the flocculus, and other parts enumerated in human anatomy, could, without difficulty, be dissected out; the superior vermiform process, also, was evident on raising up carefully the posterior lobes of the brain.

The conclusions arrived at in this short and imperfect investigation are, it will easily be seen, those which have already and for some time past been made public by Professor Huxley, viz.: 1, that the chimpanzee is not, properly speaking, quadrumanous, but that it possesses four prehensile extremities, two hands, namely, and two feet; and 2, that the brain of the chimpanzee differs from the brain of the man only in size and weight, therefore in the smaller size and extent of its cerebral convolutions; the same parts, without exception, exist in both brains. Whether the cerebral matter of the ape differs from that of man in microscopic characters, or how otherwise it may differ, are problems yet to be worked out.

On the syndactylous condition of the hand in man and the anthropoid apes. By C. CARTER BLAKE, F.G.S., F.A.S.L. (D.)—I have now the honour to call the attention of the Section to a curious abnormity which is presented by the integument of a specimen of old male gorilla which was brought from the Gaboon by Mr. W. Winwood Reade, and presented by that gentleman to the Museum of the Anthropological Society of London. The specimens of Gorilla which have been the subjects of the elaborate and complete memoirs which have appeared from the pen of MM. Duvernoy and Isidore Geoffroy St. Hilaire, in the Archives of the Paris Museum (vols. viii and x), and by Professor Owen in various parts of the *Zoological Transactions*, have, with those described by other authors, all coincided in one attributed character, true as regards the specimens with which they were acquainted, which probably represent the majority of specimens of gorilla which had been examined in Europe. This statement, reduced to a general proposition, was that the integument of the skin of the fingers was more or less connected across the first digital phalanx, in such a manner that the first joints were firmly connected together by skin, sometimes as far as the distal extremity of the first phalanx, sometimes merely to the middle of this phalanx. In no specimen of gorilla, of the description of which I am yet cognisant, are the digits of the anterior extremity free to the same extent as in man, in which the distal extremities of the metacarpals mark the termination of the amount of syndactyly of the hand. In the specimen of gorilla to which allusion is made in this short note, the digits of the fingers present a different condition of connection than in the typical specimens described by zoologists. The second (index), third (medius), and fourth (annulus) digits are free beyond the distal end of the metacarpals as in the human subject; the fifth digit (minimus) is also in a less degree attached to the annulus than in the specimens of gorilla contained in various public museums. We have thus a specimen of gorilla in which the digits of the hand are almost as free as in the hand of the lower races of mankind. Careful examination by a lens, of the integument, before the preparation of the specimen by Mr. Leadbeater, who first called my attention to this abnormity, demonstrates the fact that the epidermis covers the cutis on the inner sides of the interdigital spaces of the first phalanges of this specimen. The consistency of this epidermis merely differs in degree from that of the homologous structure in the foot, and in other parts of the body. It would be interesting to compare such a curious abnormity of the integument with the similar abnormities which exist in the human

species. The human fingers are most frequently connected together by syndactyly, and remain during life in that state of arrested development (as regards the integument) which is typified by the permanent stage of development of the gorilla. On the other hand, I have never yet met, either in the chimpanzee or oran-útan, with a similar case of freedom of digits to that here described. We must, however, recollect that the number of specimens of chimpanzee and oran-utan, which have been accurately described anatomically, form a very small percentage. How many individuals of gorilla may exist, in which a similar "accidental" variety may exist, must remain for a long time unknown to us. Syndactyly is often congenital. A case has recently come before my observation of a married female, in which the *medius* and *annulus* of both hands are firmly connected together by integument. A similar condition prevails in one of her children; another has the deformity on the right hand; whilst the youngest preserves the digits in their normal condition. The speculation whether a like rule or its converse may or may not prevail in the ape; whether it might not through generations during which the congenital defect of the gorilla, or absence of the characteristic syndactyly, might be transmitted, operate towards the production of a more prehensive form of hand, must, however, be postponed until a vaster series of specimens shall be examined by anthropologists or zoologists.

On the ligamentous action of the long muscles in man and other animals. By Dr. CLELAND (Da.)—The author pointed out that, in the human subject, maximum flexion of the hip-joint could not be obtained along with full extension of the knee, on account of the shortness of the hamstring muscles; and so also maximum flexion of the ankle-joint, along with full extension of the knee, was prevented by the shortness of the gastrocnemius muscle. This limitation of movements by the shortness of muscles, he said was best seen in the humeral region of the horse, where it was so great that very little flexion or extension of the shoulder could occur without a corresponding movement at the elbow; well-marked instances of similar interdependence of joints were to be found in other parts of the horse, and also in other animals—*e.g.*, in the legs and wings of birds. He proceeded to show that movements of that description compelled in the humeral region of the horse were exactly those most frequently and usefully employed by human beings; that the shoulder and elbow were usually flexed and extended together; that likewise in walking, leaping, &c., flexion and extension of the hip, knee, and

ankle went together; that in those movements the long muscles were not alternately contracted and extended, but kept in a state of medium contraction, very slightly altering their length, and were, therefore, evidently not the muscles which produced those movements. On the other hand, it was shown that a muscle passing over two joints, if maintaining a definite length, would cause another muscle passing over only one of them to act upon both. It was argued, that in the movements referred to, the long muscles gave force, but not velocity.

Notes upon the opening of a Cist of the Stone Age, on the Coast of the Moray Frith. By GEORGE E. ROBERTS, F.A.S.L., and Professor BUSK, F.R.S. — Mr. Roberts says that, in company with his friends Dr. Gordon and Mr. Harvey Gem, he had lately visited two mounds situated upon the sandy shores at Bannat Hill, a mile from Burghead; and after examining their contents, they turned their attention to the small cairns of rudely piled stones which lie a few yards from one of the shell middens, and which evidently marked the burial places of the tribe. Two of these were piled around small enclosed spaces, formed by the junction of four upright stones. A fragment of human jaw lying on the sand outside one of those led them to search among its contents for other bones, but unsuccessfully. The second cairn, however, with its central cist, yielded better evidence. This, like the neighbouring tomb, was a rude erection of four flat sandstone slabs, placed vertically, so as to enclose a space 30 inches long by 20 inches in width. The depth of the stone, which nearly corresponded with that of the grave, was 22 inches. Three of the stones had been slightly smoothed before use. The direction of this grave was S.S.E. by N.N.W. This, however, was of no moment, as the adjoining one differed so much in this respect as to lie at nearly right angles to it. The cavity thus formed was filled with sand, into which they dug, and presently succeeded in discovering a skeleton, which had apparently been buried in a crouching position, the legs below the knee being bent beneath the hams, and the head bowed towards the knees, *brachycephalic*, and presenting other peculiarities, which Mr. Busk had described in a note attached to the paper. From the position of the skeleton he was at first inclined to consider that the cist had never been broken into, but the absence of some few of the vertebræ and of the smaller bones, rendered this somewhat uncertain, though the disturbance, whether from curiosity or another motive, seemed to have been insignificant. He regretted, however, to add, that the box in which he

packed the bones was tampered with during its transit from Elgin to London, and some of the bones, including the lower jaw, from which precious evidence might have been obtained bearing on the *Moulin Quignon enigma*, never reached him. He had made inquiries about the matter since, but fruitlessly. No pottery or fashioned stones accompanied the skeleton.

The note by Professor Busk was to the effect that the bones had belonged apparently to a young individual about five feet eight or nine inches in height, of slight make, and no great muscular development. At first sight, from the comparative delicacy of form and want of muscular impressions, one would be inclined to regard them as those of a woman, but if so she must have been of more than the usual stature. Unfortunately no part of the pelvis, which would enable a correct judgment as to this point to be formed, was found among the remains. If the owner was a man he must have been a small size, and not of a strong build, with a remarkably small head for a male. The cranium was decidedly brachycephalic, the proportions of length to breadth being as 1·00 to ·823, and for its size rather unusually high, the proportion of that dimension being to the length as ·808 to 1·00. The forehead was narrow, and the superorbital ridges very slightly projecting, although the frontal sinuses were well developed. Compared with other ancient crania this might be regarded as belonging to the same class as those which had been considered as appertaining to the stone period of the North of Europe.

Mr. Carter Blake said that although the shortheaded proportions of the skull reminded us of the skulls of the stone period of Denmark, or of the skull which had been discovered at Kellet, in Lancashire; although the manner in which the body was entombed, with the corpse in a crouching posture, are to a certain extent in accordance with similar conditions in such ancient remains, as, *e.g.*, those from Aurignac, in the south of France; yet the undoubted association of the skeleton with bronze remains, precluded our conception of such an extreme antiquity as that which would be coeval with the formation of the shell mounds of Denmark. More recent evidences, however, acquired by Mr. Lubbock, have rewarded that inquirer, by undoubted bronze remains from kjokkenmöddings in Scotland, and the important facts which Mr. Roberts and Professor Busk had laid before the Section, have for the first time given us reliable evidence respecting the physical characters of these old prehistoric inhabitants of Scotland. Although the harsher features which characterized the skulls of the Danish stone period are softened, yet there is just so much family likeness as to lead us to the conclusion that one and the same general type of man inhabited Northern Europe, in Denmark, before the Baltic had so changed its beds, as to be no longer capable of

supporting those especially marine mollusca, which have passed away since the advent of our hunting and fishing population on its shores.

J. CRAWFURD, Esq., F.R.S., *On the Commixture of the Races of Man as affecting the Progress of Civilization.*—It was not until the discovery of a new world that races of man of strikingly contrasted qualities came to intermix. In the western world, the intermixture of nations which followed the conquests—first of the Romans, and afterwards of the northern nations—was an union of races of equal quality; and hence it cannot be predicated that either improvement or deterioration was the result. Very different was the case in the eastern world. There Greeks, Romans, and Goths intermingled with races greatly inferior to themselves—such as Egyptians and Syrians—and hence the deterioration to which, in a great measure, must be ascribed that decline in civilization which ended in the downfall of the Roman power. Nature has endowed the various races of man with widely different qualities, bodily and mental, much in the same way as it has done with several closely allied species of the lower animals. When the qualities of different races of man are equal, no detriment results from their union. The mongrel French and English are equal to the pure breeds of Germany and Scandinavia. When, on the other hand, they are unequal, deterioration of the higher race is the inevitable result. When the disparity of races is extreme, no amalgamation at all takes place, for an antipathy is the result, somewhat similar to that which prevents admixture between closely allied species of the lower animals in the wild state. The Hottentots, the Caffres, and the Negroes of Southern Africa have lived immemorially side by side without crossing. The author then remarked that the antipathy of race is presented in the greatest intensity and on the largest scale in the new world, the highest and lowest types of man being there brought face to face. The author then alluded to the laws of several of the American states with regard to Negroes, and stated his opinion that it is the presence of this African race, too prone to live and labour in slavery or in social degradation, and utterly incapable of rising to an equality with the higher race among whom it has been planted, that has caused the present distracted state of the North-American continent.

Mr. CRAWFURD's Second Paper commenced with the *Mongolian Race.* As the race seems one throughout, although, by alternations of invasions and conquests, no doubt considerable intermixture must have taken place, no appreciable difference, whether in physical form or intellectual capacity, has followed. Towards the western frontier, however, there seems to have been some commixture with the Hindus,

as in the example of Assam; and, towards the Eastern, with the Chinese, as in the case of the people of Anam. The Indo-Chinese have, however, commingled with the Chinese in certain localities. The admixture, in these instances, is of comparatively modern date, not reaching further back than the first intimate knowledge of the nations of the far East by Europeans. It has arisen from Chinese emigration, originating in the pressure of population on subsistence. As is well known, Chinese emigration is, with very trifling exceptions, confined to adult males, and these always of the working classes, without leaders or capitalists. The immigrants get or purchase wives in the countries in which they settle, and the result is a mixed race, always educated as the fathers,—in energy and industry below the Chinese, but far above the Indo-Chinese. Fresh immigrants find wives among these half-castes; and in due time a population springs up little distinguished from the pure Chinese, except in the possession of a better acquaintance with the country and people they are planted among than the original immigrants. To the industry and skill of these colonists, for such they virtually are, are owing nearly all the staple productions of the countries in which they are settled ministering to foreign commerce. Within the vast bounds of China, the race of man, whether situated eight degrees within the tropic, or twice that distance beyond it, seems one and the same. What is true of China is equally so of Japan, over the whole of which one peculiar race would seem to extend, the exceptions being trifling, and consisting only of the aboriginal races inhabiting the island of Jesso and the Kurile Islands, with neither of which despised races do the Japanese appear to commix. The Japanese received letters and religion from China, but no evidence exists of a colonization of Chinese in Japan; and the wide difference, in manners, language, and institutions, which exists between the two races, would seem to indicate that no considerable intermixture ever took place. Indeed, from all we know of the history of the Japanese, an intimate connexion with the Chinese has ever been repugnant to them. I proceed next to take a glance at the commixture of races which has taken place within the vast insular region which French geographers have of late designated the Oceanic, as forming a fifth division of the globe. This great portion of the globe extends north and south from Formosa to New Zealand, and from west to east from Sumatra to within two thousand miles of the American continent. The most prominent aboriginal races existing in the vast region in question, are the Malay; the pigmy Negro of the Malay Peninsula and Philippines; the stalwart Negro, such as the

people of New Guinea, New Caledonia, and the Fijis; the tall brown-complexioned people, or Polynesians, of whom the Tahitians, Hawaians, and Maories are examples; and finally the Australians. All these differ so completely in physical form, and there can be no doubt of their being different races of men. The strangers that have intermixed with the aborigines consist of Hindus, Arabs, and Europeans of the north and south of Europe. Among the native races there has been little commixture, and, with partial exceptions, none to the extent of forming a permanent cross-breed. Between the pigmy Negroes and the Malayans, although dwelling in the same countries, sexual unions seem no more to take place than between closely allied species of the lower animals in the state of nature. It is stated, however, that between the tall Negroes of New Guinea, with its adjacent islands, and the Malayan settlers on their coast, a cross-breed has sprung up. The people of the Fiji group afford an example of a cross between the tall Negro and the Polynesian, a fact to which the personal appearance of the people, and their mixed language, bear testimony. When, within the Oceanic region, the race is found to be one and the same, a difference of language as a test of race must, as in other parts of the world, go for nothing. Thus the Malays, the Javanese, some half-dozen nations of Sumatra, a dozen of Celebes, and perhaps a hundred of Borneo, speak essentially different tongues, yet are of one and the same race, or at least differ no more from each other than do Europeans, African Negroes, Hindus, native Americans, or Chinese. The earliest strangers who mixed their blood with the people of the Oceanic region were the Hindus, and, as might be looked for, it was confined to the race nearest to their own country, the Malayan, never having reached the rude and remote Polynesians and Australians, a fact sufficiently proved by the total absence in all their tongues of any trace of a Hindu language. The number of the Hindu settlers compared with the indigenous people must in the nature of things have been small, and it follows that it has left no trace of the peculiar characteristics of the Hindu physical form. The only evidence of the intercourse consists in language and relics of Hindu religion and customs, with Hindu architectural monuments. These, however, are abundant, especially in Java and Sumatra, the nearest countries to Hindustan, and also the most attractive to the emigrant, from their extent, their fertility, and most probably also from their superior indigenous civilizations. Among European nations, the Portuguese and Spaniards, the latter more especially, are the only people who have intermixed to any considerable extent with the Malayan

race, and none have done so to any appreciable degree with any of the other Oceanic races. In Malacca and Timor, the only portion of the Malay Archipelago long held by the Portuguese, a cross-race has sprung up with so much of Malay blood as to be hardly distinguishable from the Malays themselves. In the Philippines a far more considerable population has arisen from the union of the Spaniards with the natives, known, as is the cross of the Red Man with the Spaniard, by the name of Mestizoes, or hybrids. We possess one unique example of a hybrid race from the union of the European with the brown Polynesian, and have the blood of the two parties of equal amount. This is the case of the mutineers of the Bounty, who settled in the little unoccupied island of Pitcairn in 1790. In 1793 the colony consisted of the following parties:—nine Englishmen, thirteen Tahitian women, and six Tahitian men, making a total of twenty-eight persons. In 1814 they had increased to forty-eight; in 1831, to eighty-seven; in 1853 to one hundred and seventy; and in 1862, removed to Norfolk Island, Pitcairn being found too small to maintain them, they had risen to the number of two hundred and sixty-eight, so that in seventy years time the population had multiplied full nine-fold. The Tahitian men left no offspring, and as neither European nor Polynesian has from the first joined them, they may be described as pure Mestizoes, or half-castes. When seen in 1814, a few of the members of this peculiar colony were of the dark complexion of the first mothers, but the majority, following the physical characters of the fathers, were not to be distinguished from the inhabitants of an ordinary English village. As at present settled in Norfolk Island, they are found to be wanting in the energy and enterprise of their paternal forefathers. A noticeable fact connected with this little community is the rapid increase of population, and this without any addition by immigration. It is a contrast to the stationary or retrograde state of population in the other islands of the Pacific. The difference, no doubt, has arisen from superiority of race and civilization; and although the last of these had no higher source than a midshipman and eight English sailors, it was sufficient to generate intelligence and industry, and to exempt the colonists from the social vices which elsewhere hinder the advance of population.

Professor Wilson said that he had devoted a great deal of time to the question of the mixture of races on the North American Continent. In the case of the Negro the subject was surrounded by so many social difficulties that so far as important ethnological results were concerned, it could scarcely be said to have had a fair trial. Not so, however, in the case of the red man. There was no legal impediment to marriage between a Red Indian and a white man;

some of the latter even boasted of their Indian descent. The colonists who went from this country to the North American Continent in the early years of settlement were generally young, unmarried men, who united themselves, either with or without marriage, to Indian women. In process of time there arose a vast population possessing English and Indian blood in their veins—such, for instance, as was found on the Red River settlement. These people possessed physical qualities of a high order—were persevering in the chase, and valiant in fight with their enemies. They were, however, to some extent, civilised, being chiefly Roman Catholics in religion, and no longer using the scalping knife upon their vanquished foes. When introduced into the society of Anglo-Saxons they frequently manifested very superior intelligence and ability, indicating no degree of inferiority whatever as compared with what was usually called the superior race. One of these persons had been under his (Professor Wilson's) instructions at University College, had taken his B.A. degree with honours in classics, and afterwards visited England as an agent of the Red River Settlements. Assuming that the Red Indian was an inferior race, and the Anglo-Saxon one of the highest types of man, the instance he had given would go to prove that the mixture of inferior and superior races did not cause either one or the other to deteriorate. The problem could not be tried to the extent to which it was desirable to carry it, as the white men had arrived in America in such vast numbers as to drive the aborigines before them; but it did not follow that the Red Indian disappeared because he was the inferior race. The Red River settlement was an illustration of the amalgamation of the two races without such deterioration, and he (Professor Wilson) believed that there was a much larger admixture of Indian blood in the white population of the American Continent than was generally supposed; and that this accounted for some of the peculiarities of the American, as contrasted with the European character. He believed that a mixture might take place between the white and the red race, with benefit to both He thought that the same doctrine would apply with respect to the Negro and white man, though in that case there had not been so fair a trial as in the case of the Indian.

Mr. CRAFT said that being of African extraction, he felt called upon to make a few remarks upon the subject under discussion. He did not quite understand whether the author of the essay to which they had listened intended to say that no amalgamation had taken place between the Negro and the Anglo-Saxon race in the United States. He thought that Mr. Crawfurd had intimated that there was a very strong antipathy between the two races, and that the laws of the Southern States prohibited intermarriage between the Negro and the whites. He submitted that in spite of those laws there was a large population claiming affinity with both races. He thought he should be right in saying that nearly two-thirds of the Negroes in the Northern states of America had more or less of European blood in them, and he also believed that had it not been for that amalgamation, instead of there being 4,000,000 of slaves in those states there

probably would not have been more than 1,000,000. He would simply state, on behalf of the African race, that whenever they had had equal opportunities with the whites they had shown that they possessed considerable intellectual ability, and many of them had risen to very high position in society. He mentioned that, in order that persons who were not acquainted with Africans might understand that there was just as much difference between individual Africans as between individual Englishmen. He found that all Englishmen were not Shakespeares. He did not admit that Kaffirs were fair specimens of the Negro race.

Dr. JAMES HUNT agreed with the author of the essay in the general conclusions at which he had arrived, but he thought he had not dwelt sufficiently on the great physiological law which was admitted by most observers, that where the intermixture was kept up through succeeding generations the offspring gradually died out, and the race became extinct. He thought that the laws in the Southern states of America against the intermarriage of the negroes and the whites were wise laws, but he admitted that the subject was surrounded by considerable difficulties.

Mr. MARKHAM cited some instances from the Continent of South America, to prove that intermarriages between European and native American tribes tended to improve the intellectual and physical character of the population.

The discussion was continued by Professor Wilson, Mr. Carter Blake, Mr. Ralph Carr, and Mr. Craft.

Troops in India. By Dr. CAMPS (H.)—An analysis was given of the recent report on the sanitary state of our army in India. The conclusions drawn were:—1. That by far the larger proportion of the mortality and inefficiency in the Indian army has arisen from endemic diseases, and notably from fevers, diarrhœa, dysentery, cholera, and from diseases of the liver. 2. That the predisposition to these diseases is in part attributable to malaria, in conjunction with extremes of temperature, moisture, and variability. 3. But that there are other causes of a very active kind in India connected with stations, barracks, hospitals, and the habits of the men, of the same nature as those which are known in colder climates to occasion attacks of the very diseases from which the Indian army suffers so severely.

Dr. JAMES BIRD said that Dr. Camps's paper seemed to be an abstract of the Sanitary Commissioners' Report, which he contended—by leaving out the ratio between peace and war—was not correct in its statistics. The returns were mixed returns: it was absolutely necessary, in order to obtain a correct result, that the peace and war returns should be separated. He had no fear of the climate, if the sanitary measures necessary were carried out. Dr. Edward Balfour said, in 1849, that he differed entirely from Col. Sykes's conclusion, that intemperance and vice were the main causes of disease; and he

(Dr. Bird) differed from them also. The causes of the excessive mortality were heat, moisture, and localities. The Station Reports showed that; although vice and intemperance had their effect. The colonel seemed to think that the rate of mortality in India was 67·9. He had shown that in the last twenty-six years it had scarcely exceeded 44, and in the last five years it had not exceeded 35. He dissented from the colonel's opinion as to the excellence of the barracks. The great mortality was increased by ill-ventilated barracks, and the filthy cess-pools in the midst of them. He believed the proper remedial measures applied to them would cause life to be preserved in India as well as in any other country. He held that the respiratory functions of the human body could be acclimatized to a warm region; but it was impossible to acclimatize any human body to miasmata; and he fully believed that, in order to lessen the mortality of the troops in India, it would be necessary to lay a good foundation for the barracks, and attend to their arrangements as carefully as those of workhouses and hospitals in this country were made.

Dr. JAMES HUNT entirely disagreed with Dr. Bird on the subject of acclimatization. There was a physiological change produced; but it was not acclimatization, but the gradual production of disease. With regard to the fact of the mortality being chiefly put down to intemperance and immorality, he must say he could find no evidence of that. It was certain that in such climates as that of India, it was necessary for European inhabitants to take stimulants; the defence of teetotalism for India was objectionable. He held that there should be a judicious selection of men suited to hot climates; they could not preserve every one in health there. As for attempting to rear the children of European parents, the system was utterly false. Throughout the whole of Bengal, there was not the third generation of Europeans; the mortality among children was excessive, and, in fact, it was utterly impossible to rear children. His conclusion was, that the only way to cause a decrease of mortality among the troops, would be the selection of men suitable for the climate. By a study of temperaments and other peculiarities, it was possible to predict with a degree of certainty, which he found most surprising, what would be the influence of climate on different temperaments.

Colonel SYKES defended the Commission from the statements of Dr. Bird. For their reports they had the authority of a very great number of witnesses, and there could be no impeachment of the integrity of its members. Where great heat and moisture existed, disease prevailed; but he found that where great heat prevailed along with dryness, it was not detrimental to the health of the men. The great evil of the whole system, was the employment of European troops in such numbers without real necessity, thus causing an enormous amount of misery amongst the families of the labouring poor in England. That was what most of all he deplored. A very great deal of expense had been incurred in barrack accommodation; and he was still of opinion that vice and intemperance were fruitful sources of disease. After considering what we had lost, the question for them was, what were we likely to lose in the future? At all events, we

should preserve our power in India with the very smallest possible number of English troops; and he should even be inclined for us to run some risk for the sake of humanity, and for the preservation of the youthful blood and sinew of the country.

Dr. HANCOCK was of opinion that vice was a great source of the disease in India, but that was created in a great measure by the restrictions on marriage. The climate was not the cause. The arbitrary restrictions on the marriage of the men deprived them alike of friends and family, and they were driven to the vices which ultimately brought them to the hospitals.

Colonel BAKER thought that the conclusion drawn from the papers before them, that the average 67.9 per thousand was the true average of the mortality of troops in India, was erroneous. He maintained that the sanitary measures of the Government in India had been very effective, and had reduced the average mortality in time of peace.

Instinctive Actions. Dr. WM. MURRAY (D *a*) endeavoured to demonstrate the part which each of the sources of nervous power plays in generating those effects, which, in the aggregate, make up an instinctive act or set of actions. The author thought that the instinctive movements of animals, and their nerves or physical construction, do not differ from those of man in kind, but in degree. In man the volitional, as representing reason, abstract deduction, and experience, is immensely superior to the others. As we descend in the social scale, we find the emotional, as the originator of the purely and really instinctive movements, become more prominent, and generally carrying the will with it, for we very seldom see an animal going contrary to his instincts. Lower still in the scale, we find all the arts necessary to the life of the animal left to the care and control of reflex action. Was there, then, he asked, in animals an intelligence? And his reply was—We strongly incline to the belief that there is, and that it varies in its power with the kind of animal, and manifests its existence by the extent to which it controls the emotional or purely instinctive part of his actions.

Dr. B. W. RICHARDSON rejoiced that the time had come when the convictions of men of science could be freely expressed, and when they dared to assert that there was nothing in man that might not be understood. Physiologists ought not to admit that there was any hidden vital force or mysterious entity in man which could not be comprehended or explained. For his own part, he would go still further than Dr. Murray had gone, and assert every influence upon man to be from without. Men were moved and controlled by the eternal universe alone; and a Shakespeare himself, in grandest poetic effort, was but giving up through his hands that which he had received and concentrated from the universe around him.

On Life in the Atmosphere. By JAMES SAMUELSON, Esq. (D *a*.)—

No subject in natural history, he remarked, except the allied one, the origin of species, had of late excited greater interest in the scientific world than the origin of the lowest types of living beings on the globe; and although the problem was far from being solved, yet, the investigations that had accompanied the discussion had already served the useful purpose of throwing new light on the anatomy and life history of the mysterious little forms of which it treated. It was rather with the latter object, than in the expectation of being able to assist in the solution of the general question, that he ventured to lay before the association the results of investigations recently made. He had, for example, taken rags imported from various countries, and shaken the dust from them into distilled water, which he then exposed to the atmosphere; and, after describing generally the character of the living forms he had discovered in this pure water, he stated in detail the forms of life found in each kind of dust, and among these were some new species of rhizopoda and infusoria, and an interesting ciliated worm-shaped form, which he believed to be a collection of the larvæ of some other infusoria. The general results of the microscropical examination of these fluids between the 27th of July and 15 of August was as follows:—in the dust of Egypt, Japan, Melbourne, and Trieste, life was the most abundant, and the development of the different forms was rapid. In conclusion, he observed that if he was correct in supposing the germs of the living forms that he had described to be present in the dust conveyed by the atmosphere, and in distilled water, it was worthy of notice that these germs retain vitality for a long period, one of which he could not pretend to define the limit. In his experience they outlived the heat of a tropical sun, and the dryness of a warm room during the whole winter; but in Dr. Pouchet's case they retained their life 2,000 years, for he obtained his from the interior of the pyramids of Egypt, and then survived an oil of 400 degrees of heat. Mr. Samuelson endeavoured to disprove the theory of spontaneous generation. He suggested whether the great rapidity with which these germs are multiplied might not account for the spread of epidemic diseases. He did not profess to have any acquaintance with such diseases; but might it not be desirable to subject the atmosphere of hospitals to the microscopic test?

Dr. Rolleston attempted to ridicule the notion of spontaneous generation which certain French writers had propounded and endeavoured to defend.

Dr. B. W. Richardson believed it might yet be discovered that

certain diseases of an epidemic nature were produced by the infusoria of the atmosphere as suggested by Mr. Samuelson, and thought that the best way of arriving at some conclusion upon the subject would be by first removing all those diseases which all agreed could not possibly be so engendered, and testing the remainder. There would not be much difficulty in this, because all our known diseases did not exceed 279; and of these there were not more than ten or twelve not already accounted for. Many facts had been related confirmatory of the opinion that epidemic diseases were sometimes produced by inhaling dust filled with living creatures. On one occasion, for instance, three men were thrashing in a barn. In turning over a truss of straw some dust rose in their faces, and caused a slight irritation in the nose and throat. They thought nothing of it, however, but presently one of them said he felt cold, then another, and then the third; then one felt sick, and in the course of an hour they were all unwell, and on the following day all three were seized with erysipelas. Dr. Sailsby had recently made several experiments of this order. Some men were handling a tree covered with a fungus, when they were seized with symptoms resembling measles. The doctor first inoculated himself and then his children with the virus, and like results followed; they had the measles, or something very similar. The same facts had been observed by Dr. M'Donald, who had learnt of a case in which measles were produced in a boy by some slightly decomposed linseed meal having been thrown in his face. He strongly advised that the subject should be duly investigated.

Influence of high altitudes on man. (A.)—Mr. GLAISHER gave some account of the curious changes in colour that he and Mr. Coxwell experienced in ascending, and remarked that they could now go a mile higher without turning quite so blue as before.

Professor OWEN said he had attended the section chiefly in the hope of hearing from Mr. Glaisher something of the influences of these very high distances on the human frame, which was adapted, of course, to a very different medium. The fact which Mr. Glaisher mentioned as to his feeling a greater power of resisting the influence of very high temperatures was very interesting in physiology, and in relation to the series of facts with which they were acquainted. They knew their lungs did adapt themselves to atmospheres of different degrees of gravity, so that there were people who lived habitually on high mountains and felt no difficulty in breathing such as was felt at once when the inhabitant of a plain or low country came up to these elevations. Now, that depended upon the greater proportion of the minute cells of the lungs which are open and receive an attenuated atmosphere, in proportion to the minute cells that are occupied by a quantity of mucus. Those on the plain did not make so large a use of their breathing apparatus as those who lived at great altitudes. Hence more cells, occupied by mucus, would be taken up, and opened to free course and play; and he had no doubt that was the solution of the interesting fact mentioned by Mr. Glaisher. Physiolo-

gists were all agreed that one condition of longevity was the capacity of the chest, and therefore he hoped the increased breathing capacity acquired by Messrs. Glaisher and Coxwell would tend to the prolongation of their lives.

Mr. GLAISHER, alluding to what had fallen from Professor Owen, remarked that the adaptation of nature was certainly something wonderful. He had been in a position, anxious to remain, and though not a second was allowed him, he looked down, around, above, everywhere in a momentary glance, and everything became fixed on the retina with such ten-fold impressions that there were many of these scenes he could call to mind now, and could (if he were able to draw) reproduce them on paper. One sensation he found was that his arms were forced back and more air was taken into the chest. The eyes were clear, the brain active, and the powers increased according to the exigencies of the case.

On the aboriginal occupations of North Tynedale and Western Northumberland—an illustration of the social life of the Celts. By Rev. G. R. HALL. The present brief account embodies the results of recent researches connected with the aboriginal settlements of the western parts of Northumberland. The subject is one of more than local interest, as the ancient remains are concluded, with good reason, to be those of the earliest race of men who inhabited these islands, of whose social condition any trustworthy vestiges remain.

These dalesmen and mountaineers of a prehistoric Northumbria gave those names to hills and streams which are still current in the county. Its chief river—the Tyne—was so called long before the vanguard of the victorious Roman legions set foot upon its northern bank, or built the walls of Pons Ælii, on whose site this important town of Newcastle now stands. The name of the Tyne is usually derived from the ancient British word *tyn*, that is, the *double* river, in allusion to the two branches called the North and South Tyne, which form it.

The former branch, the North Tyne, is the larger of the two, both as to volume, and the distances from its sources in the recesses of the Cheviot range of mountains to its confluence with the sister-stream of the South Tyne. North Tynedale is about forty miles in length; and the area embraced in this notice of it, including the districts drained by its tributaries, is over three hundred square miles.

A few notes on the aboriginal occupation of North Tynedale, derived from personal research during the past and the present year, may not be devoid of interest in connection with the Geographical and Ethnological sections of the British Association.

Two kinds of ancient British caerau, or fortified towns, may be

named the hill forts and the lowland fastnesses. They are located in the strongest positions, on the top of lofty upland ridges, crags, or rounded heights, in the case of the former; and the latter occupy advantageous sites on escarpments and platforms of the lower grounds, generally flanked by deep ravines and the precipitous banks of the numerous mountain streams in the river basin itself. In one instance, the middle of an inaccessible morass or bog has been chosen near the basalt cliffs at Sewingshields; and at Bridge House, near Wark, another singular site has been selected in the hollow or sinus, shut in on every side, the remains of which are still called the "Campsteads," as the oppida, or towns themselves, are commonly termed "camps" by the dalesmen; perhaps from having been used as encampments in the mediæval border forays and conflicts.

One of the finest examples of the hill-fastness of the aboriginal Britons of this district is found on Warden Hill, overlooking the junction of the South with the North Tyne. Its height above the sea, about 600 feet, gives no inadequate idea of the extensive view obtained from this spot, and of its consequent importance as a post of observation. It commands a prospect of many miles of each of the three valleys to the north, west, and east, as they spread out beneath the eye of the spectator like radii from a common centre. The ancient town of Hexham is in the lower foreground. A broad expanse of the undulating uplands on every side can be discerned. No neighbouring eminence commands it. All around the ground slopes gently or abruptly from the apex of the hill on which the fort was built. Its area, including the three concentric ramparts by which it has been defended, is about two acres. Within these outer defences some of the aboriginal dwellings or house-circles may be traced. On the opposite bank of the North Tyne, south of the line of Hadrian's vallum, stands another fortress, built—like most of the settlements which I have noticed in the district—of massive unhewn blocks of the native freestone. If we follow the upland "wastes," the "vastæ" west of the river, which Camden tells us were inhabited in his time, as they were no doubt in the pre-historic period, by a race of nomades, half shepherds and half soldiers, "*militare genus hominum*," a few noteworthy forts can be seen, besides two or three situated to the south of the Roman Wall, the principal of which was the strong hill-fastness of Barcombe. At Sewingshields, Lonbrough, the Catlass Ridge, and Roses Bower or Wark's Burn, at Bridge House, the Stone Folds, and Leek Hill, and Hindridge, we meet with these primitive fortifications; vestiges of whose Cyclopean walls, less or

more perfect, still remain. On the eastern bank of the river, North Tyne, and the banks of the Rede, several of these hill-fastnesses exist; as Errington Hill Head and High Shield Green "Night Folds," wherein, as we may suppose, from this traditional name, the aboriginal herdsmen, like those of Casivellaunus, when threatened by the advance of Cæsar's hostile legions, were wont to secure their flocks and cattle, brought in from these upland pasture grounds (which are still noted in these parts as an excellent "summer feed") and thus protected them from the ravages of wolves and night robbers. A chain of these fortresses again, crown the elevated ridge near Otterburn, one of which was occupied by the Scots in 1538 before the famous battle of Chevy Chase, between Douglas and Percy, was fought.

We may now turn to what I have called the lowland fastnesses of the aboriginal Britons, that is, the towns whose sites are on the declivities of the valley itself.

In former times the margins of the rivers North Tyne and Rede were clothed with abundant vegetation, and many picturesque remains of the primeval woods continue to adorn these valleys in the neighbourhood of Keilder Castle, Heselyside, and Redesmouth. In clearings of the forests the aboriginal inhabitants appear to have had numerous settlements in this district. Near Keilder some very perfect examples may be noticed, where eleven of these towns existed. Amongst these the double fort, popularly known as "Bran's Walls" (a name which calls to the mind the famous Celtic chieftain of later days, Bran Galed, or Bran the Hardy) is worthy of inspection. This stronghold occupies the slope of a hill overlooking the sources of the Keilder river, and is situated in the midst of a vast amphitheatre compassed by wild heather-clad mountains, of which Pell Fell, 1,290 feet above the sea level, is the highest. The shape is that of two irregular ellipses, closely connected with projecting works, covering together about an acre of ground. Eleven hut-circles, or dwellings, can be plainly traced within, their average diameter being about twenty-four feet. The Bell's Hunkin fort, in the North Tyne Valley, about one mile west from Keilder Castle, is also in excellent preservation. It occupies a strong defensive position on the brow of a deep ravine, the site being covered with a natural growth of the birch and alder, and mountain ash. A vallum, constructed of large stones, surrounds it, but there is no fosse. The vallum rises in some places to the height of seven feet, and within it I noticed five circular foundations of primitive dwellings, measuring five, six, seven, ten, and eleven paces

across, respectively. This stronghold is erroneously spoken of by the dalesmen as a Druidical circle or temple, of which none now exist in the valley, though Wallis mentions one as having stood near Nunwick, and tradition points to another, where a solitary pillar of basalt stands near the village of Barrasford, to which a wild legend, like the Breton folklore, representing the "Palet de Gargantua," or Gargantua's quoit, is attached. A group of aboriginal fortified towns, equally numerous, exists in the vicinity of Birtley, at the angle formed by the confluence of the North Tyne and Rede. In an area of about twenty square miles I have noticed nearly as many ancient forts, situate on the declivity of the valleys west of the watershed of the Wansbeck river and the line of the Watling Street. Within a radius of two miles from Birtley six aboriginal settlements occur. The chief of these stands on a wedge-like platform, in the Countess Park Woods, strongly defended by two concurrent ravines. The site covers, including the outer ramparts and fosse, more than three acres. It is traditionally the site of a great battle in the "troublesome times," which, as this popular term implies, have left an indefaceable memory behind them. This fastness is well worthy of inspection. It is the largest and one of the most perfect illustrations of this class of these primitive works to be found in Northumberland. One of the circular huts, perhaps the residence of the chief, is of extraordinary dimensions, being forty-six feet in diameter. The foundations of the inner and outer ramparts—which are several feet high in some places—and of the dwellings within, are plainly visible. In shape it is irregularly rhomboidal—that is four sides in straight lines, well rounded off at each corner. This and the neighbouring strongholds in the valley-basin would originally correspond very closely to Strabo's description, who informs us that "the woods of the Britons are their cities, for when they have enclosed a large circuit with felled trees, they build within it houses for themselves, and hovels for their cattle." This is analogous to the practice of Englishmen of the present day, who find themselves obliged to inhabit the backwoods of Canada, or the bush of the great Australian continent. The materials used in the construction of the lowland fastnesses, as well as of the hill fortresses, so far as I have noticed, are the native freestone. Only in one instance of a series of towns on the southern slope of basalt crags at Gunnerton, has the whinstone been used. Both are built up into Cyclopean walls of unhewn but massive stones. I have observed in several sites traces of the action of fire, the white freestone having become reddened, a proof of a long occupation by the aborigines. The hut-

circles seldom vary in form, generally retaining the round shape of the primitive moveable tents of the nomadic races, of which these are continuations and reminiscences. Probably the foundations only were of stone, and the walls, ten or twelve feet high, with conical roofs, were of wattle work, roughly stuccoed perhaps on the outside, and with the skins of wild beasts, the spoils of hunting, hung within. Bede describes such a dwelling of the Britons of the seventh century, when he tells a story (*Eccles. His.* chap. 10, p. 125) of a pilgrim returning from King Oswald's grave at Maserfield, who being benighted seeks hospitality therein. The house was of wattle work chiefly; and he tells us that, after much feasting, some spark ignited the roof, where the only opening for the admission of light or the egress of smoke was situated. The British maidens were adepts at twining with their slender fingers the withies that grew by the margin of the streams and ponds into such wattle work. Their basgeds, whence our word basket, were known and valued even in the Imperial city of Rome. Circular and square huts, such as existed in Gaul, are represented on the Antonine Column. The aborigines of the North Tynedale district and the west of Northumberland were evidently somewhat above the condition of savages. They did not wholly depend for subsistence on the casual produce of the chase. At three different places a series of terrace-lines or platforms, raised one above the other, still remain in the vicinity of the ancient towns. Near Falstone, and at Birtley, they can be plainly seen. In the latter district the forts are numerous, two declivities are, as it were, furrowed by these lines of primitive culture. One, near to the Countess Park "camp," consists of eight terraces. But the largest is on the slopes of the elevated ground above the Cary House fort. The latter is in the form of two sides of a parallelogram, which, if defined on the other sides, would enclose an area of ten or twelve acres. Six terraces face north-west, running about four hundred and fifty yards, and meet the other lines, which are about half this length. Each terrace varies in height and breadth, from ten yards to a few feet broad, and in some places they are ten feet high. Here the corn, to supply the tribe with a store for present or future use, was grown. The querns, or hand-mills, were used to prepare it, of which many specimens have been found of granite and other hard materials, on different sites. Within the memory of very aged men these hand-mills were still used.

Another noteworthy feature of the district consists in the immense heaps of slag or scoriæ of iron, which I have met with on the slopes

of the vale of the North Tyne. Many hundreds of tons of this refuse from ancient ironstone smelting may be seen in a woodland glade near Birtley, and close to two primitive forts and the last named range of terrace-lines. They are still called the "Cinder Kiln Hills."

Iron exists in abundance almost at the surface of the ground. This is proved by the number of chalybeate springs, and the fact that the smaller drainage tiles are soon rendered useless by the speedy incrustation of oxide of iron that gathers on the inner surface, from the water which holds the iron in solution. From these workings, no doubt, the long, pointless swords of the ancient inhabitants were derived, though their manufacture was rude enough, from the excessive waste evident in the process. Iron spear heads have been found on the sites of some of the forts in the Keilder district. But the flint arrow and spear heads, of which a manufactory, as I consider it to have been, was noticed in removing, for purposes of tillage, the stones of a fort near Wark village, seem to have been the usual weapons previous to the Roman-Celtic period. A copper battle-axe, with beautiful scorings, was discovered, however, last year near Corbridge.

The last class of aboriginal remains to be noticed consist of the crugan, barrows or burial mounds. Several mounds of interment have existed either within or at the chief entrance of forts, which are, as usual, facing the east, the cardinal point of the heavens to many primitive races, as the north is to us. Thus at Warden Hill, outside the entrance, is a large tumulus; and in the Carey House fort, near Birtley, a kistvaen, or four upright slabs in the shape of a chest, containing a scored vase, with the ashes of some chieftain whose body had been burnt, was found by some men draining the site a few years since, who at first fancied the vessel contained hidden treasure. On the level platform above the High Shields Green "night folds" are numerous mounds in the midst of the traces of ancient culture, evidently of later date than the mounds themselves. Under each of these, tradition asserts that a warrior sleeps. The largest is called Dan's Cairn, a reminiscence, perhaps, of the terrible era of the Danish incursions into Northumbria. But the most remarkable of these tumuli exists near Gunnerton, in the neighbourhood of the "Camp Hill," where a fort has stood, noticed by Mr. MacLauchlan. The barrow itself has not been noticed or described. It is a conical mound, thirty feet high, surrounded by a deep fosse and vallum, with covered way leading to the stream on whose precipitous banks it stands. The circumference is about one hundred paces. A wild legend, like those of the Scandinavian Sagas, is connected with it of

some supernatural being, usually termed a dragon, keeping watch over a secret hoard at its centre. At some distinct date there has been an attempt to learn the truth of this tradition. The mound, which is still called the Gunnerton Money Hill, has been scooped out to a considerable depth from the top and eastern side.

These castrametations—terrace lines of field culture workings for iron—and burial mounds are the chief vestiges remaining in this part of Western Northumberland of its aboriginal inhabitants. They are interesting relics of a departed race who peopled this island in times before the dawn of history. When we ask who the builders of these cyclopean structures were, and when they lived, we are unable to give any definite reply. Most probably they were a portion of the Japhetan family, who, as Mr. Wright informs us, first overspread the countries and islands of Western Europe. When we compare the characteristics of these early remains with those by antiquaries elsewhere in England and Scotland, we may, perhaps, conclude that these ancient Northumbrians were of the Celtic race. If we follow the guidance of Ptolemy and other ancient geographers, we may further decide that the tribe or nation was that of the Gadeni, whose exact tribal boundaries are not accurately ascertained, but who appear to have peopled the western parts of Northumberland, the small portion of Cumberland north of the river Irthing, the western part of Roxburghshire, the whole of the county of Selkirk, with Tweeddale, a great part of Mid-Lothian, and nearly all West Lothian. This warlike and powerful tribe had the Brigantes as their neighbours south of the Tyne, and the Ottadeni eastwards on the coast of the German Ocean. These wild, inaccessible regions of mountain and moorland were well calculated to develope a spirit of bravery and independence. And thus we find that when the Emperor Severus invaded these parts, his legions were appalled at the strength, activity, and ferocity of the Northern Britons.

From a comparison of the materials used in the construction of the hill-forts and lowland fastnesses of this district, and their general conformation, I cannot find any sufficient warrant, as far as this region is concerned, for the supposition entertained by the Rev. W. Barnes (*Notes on Ancient Britain*, p. 41), that the two classes of fortifications were built and occupied at different, and perhaps, widely distant times. He remarks, "These wood-fastnesses (of which Cæsar and Strabo speak), seem to have been the trefydd in the lowlands and nor the caeran of our hills, which might have been antiquities even in those days"—the days of the Roman invasion and colonization of Britain.

As to the social life of these remote times, perhaps, the closest analogy and resemblance will be met with in the condition of the North American Indians, amongst whom Dr. Wilson (*Prehistoric Man*, vol. i, chap. i, p. 6) conceives that he has found "the living present of the long obliterated past of the Allophylian of Britain's prehistoric ages."

Mr. GEORGE TATE, F.G.S., etc., said that, having for several years investigated the early antiquities of Northumberland, he would respond to the call of the president. The paper read, gave an interesting description of the relative position of several of the ancient remains in Tynedale; but the author had not produced the best kind of evidence to support his speculations; nor did he seem acquainted with the facts elicited by better methods of investigation, than he had adopted. If we are to arrive at satisfactory conclusions as to the age of such antiquities, and as to the character of the people of whom they are the remains, archæologists must follow the course pursued by the geologist, who is not content merely to look at the outside of a rock and then to speculate on its age and origin; but, with hammer in hand, he will spend days and even weeks in breaking it up to find the organisms it contains, and from these he can arrive at sound conclusions both as to its age and the conditions under which it was formed. In like manner, must the antiquary condescend to use the spade and the pickaxe, to open the barrow and clear away the *débris* from ruined hut and camp, so that he may find relics which may throw light on the age of the structure and on the character of the people to whom they originally belonged. By this method, extensive explorations were made during the summers of 1861 and 1862 into the ancient British remains among the Cheviots, in the valley of the Breamish and on Yevering Bell and its neighbourhood. The Berwickshire Naturalists' Club were enabled to make these investigations, through the liberality of His Grace the Duke of Northumberland; and two reports have been printed in the *Transactions* of that body, giving an account of the explorations. Before referring further to these, I may notice that the author of the paper speaks of a copper battle-axe found near Corbridge; but this instrument is a bronze celt, peculiar for its ornamentation, but not found in association with any ancient British remains. He also calls a house described by Bede, British, while there seems to be little doubt of its being one of the ordinary Saxon dwellings. The ancient British remains examined by excavations, were in the wild hilly districts of North Northumberland, and consisted of fortified towns, the oppida of that people, great forts which crested the higher hills; smaller yet strong fortlets on the slopes of the hills and in the high valleys; hut circles, which were within towns and forts and scattered around the lesser fortlets; and barrows, the sepulchres of this people. A fortified town and ancient British oppidum, at Greaves Ash, near Linhope, is the most remarkable of these antiquities; its ruins cover an area of about twenty acres. Situated in the valley of the Breamish, on elevated ground, tolerably

level, and with the high hill of Greenshaw sheltering it on the north; it consists of three principal parts at a little distance from each other, but all so connected by intervening buildings and defensive works as to form one town. The most important of these divisions consists of a number of hut circles, within a large circle, defended by two massive stone walls, one being from five to seven feet, and the outer one being from ten to twelve feet in thickness, and nearly a thousand feet in circumference. The explorations revealed peculiarities of structure in these great cyclopean walls, which are fully described in the published reports. The walls of the huts exhibited the same kind of masonry; for the most part, they were quite circular, varying in diameter from eleven to twenty-seven feet, each with a well-formed entrance facing the east or south-east. Many of them were flagged with flat porphyry stones, and, in some cases, a raised row of stones across the entrance had acted as a check to a door which had opened inwards. The fire had been in the centre, as hearthstones were noticed still retaining the marks of fire, and in a similar structure near Yevering, charred wood was discovered on a central hearthstone. In one hut a rude low stone bench was observed, which may have been used as a bed. Some portions of the great defensive walls still remain five feet in height; and in some parts the walls of the hut circles are from two to three feet high. Originally, the former may have been from eight to ten feet, and the latter as much as five feet in height, probably a tapering roof, of timber, wattle-work, and sods or rushes rested on the walls of the huts. Forts, strong from their natural position, and from the thickness of their ramparts, crown many of the higher hills; perhaps the most remarkable is that of Yevering, a fine conical hill about fifteen hundred feet high. The summit is girt round by a thick wall, now broken down, enclosing an area, one thousand yards in circumference. Wild fancies were entertained by the antiquaries of last century respecting this place; it was with them a Druidical temple, and one, of keener sight or stronger imagination than others, saw an altar stone with the marks of fire, which had burnt human victims. Such notions are entirely groundless. Too high and exposed to be suited for permanent residence, Yevering might be occasionally inhabited during the summer, but like others in similar situations, it was a place of refuge and defence for the people inhabiting the valleys, when they were attacked by a too powerful enemy. The people lived on the slopes of the hills, and in high and dry valleys between these hills, in circular huts, protected partly by smaller strongholds, the residences of the chiefs. These strongholds or fortlets were from thirty to one hundred and fifty feet in diameter, with thick walls similar to those at Greaves Ash; and the huts were scattered around them; some of which were little more than pit dwellings, dug out of the hill-side. The antiquities described, in the paper read, are similar to those among the Cheviot hills; and it is noticeable, that they are similar to what are found in the south-west of England, in Cornwall, and in some parts of Scotland, leading to the inference, that, at an early period, the whole of the island was occupied by the same race. The arrangements, however, tell of a divided state of

society—of separate tribes and clans, often at war with each other. Exposure to similar dangers in different periods, causes the adoption of similar methods of meeting these dangers, modified, however, by different degrees in civilization. The relation of the several kinds of antiquities to each other seems analogous to what we have in the middle ages on the Borders, where there was almost constant warfare. The great hill forts are like the baronial castles; and as the cottages of Northumbrians nestled under the protection of the mediæval pele, so were the ruder huts of the ancient Britons scattered in the valleys around or near to the strong fortlets. It has been asked what is the age of these antiquities. Sir Gardner Wilkinson is of opinion that the ancient Britons, alarmed by the first invasion by the Romans, erected such strong fortifications as that at Greaves Ash to protect themselves against this new and formidable enemy. Taking, however, the relics discovered as a guide, as well as peculiarities of structure, I think, they are referable to a much earlier period. Both in the valley of the Breamish, on Yevering itself, and in the fortlets in the valley, several flint weapons and instruments and a quantity of broken pottery of the coarsest kind were found; in one of the huts was discovered a portion of an armlet of white opalised glass, similar to some obtained in Switzerland, belonging to the so-called bronze age, and similar to others from Scotland, taken out of a moss, and from under a cairn. A portion of an armlet of variously coloured glass was got out of a hut at Yevering, and a fine green glass bead out of a hut in the Breamish. Other armlets made of polished oak were in a hut on the top of Yevering. And within the same great fort was found a copper pin, which appeared to have been part of a fibula. There were no golden ornaments. The bones of a horse and the horns of the red deer were found in the valley of the Breamish. When these cities, forts, and dwellings were first constructed, the ancient Britons were far from being in a savage state; they had a rude civilization of their own, and had made some advance in art. Though their glass ornaments may have been imported from Phœnicia, they made their own bronze and flint weapons and instruments, and their domestic vessels of clay. Hunters they were doubtless, and keepers of cattle; but some of them, at least, had settled within walled towns, and were, to some extent, employed in cultivating the soil and raising corn—probably oats, for from the teeth of an Ancient Briton, taken out of a cist at Tossin, being worn flat in the crown, he may have lived upon hard vegetable feed. Numbers of querns of a primitive type, found over the district explored, prove that corn was ground and used; and as the under stone of one of these querns was found, as a flag in a hut, we have evidence of the use of hand-mills by the early occupants. Terraces on the hill sides, the most remarkable of which are at Heathpool, seem to me to mark the spots of this ancient cultivation, which indeed could only be carried on at moderate heights among the hills, since much of the lower parts of the country would be covered with swamps and woods. Two reasons would cause cultivation to be practised on horizontal terraces at an early period. Less mechanical power would be required to form ridges along the

side of the hill, and the heavy rainfalls of the mountainous regions would be less liable to wash away the soil from ridges that were horizontal, than from those up and down the hill. Many barrows had been opened; and in one of them near to Yevering Bell, an interesting discovery was made, tending to prove that iron was known to, and used by the Ancient Britons, at a period earlier than archæologists are willing to admit. In this barrow, along with flint weapons and instruments, and potsherds, some lumps of iron slag were found; and this discovery throws some light on the origin of heaps of iron slag, which I have met with in several places in the wild moorlands, distant from modern habitations. Respecting them there are no traditions, and connected with them are no remains of furnaces or other buildings; but they are not far distant from ancient British camps and forts. It is, therefore, not improbable that these heaps mark the spots where ancient Britons smelted the iron-stones of the district. In a cist at Tossin an iron weapon was found, associated with urns scored with the zigzag pattern, characteristic of ancient British interments. Of this I have given a description in the *Proceedings* of the Society of Antiquaries for Scotland. Probably enough, iron was more in use than is generally supposed; for from its rapid oxidation, it is only in very dry situations that it can be preserved. Such facts throw serious doubts over the Scandinavian classification of antiquities into stone, bronze, and iron ages. Certainly they can apply only in a very general and indefinite manner to Northumberland. Weapons of stone, wood, or bone may in many parts of the world have been used before those of metal; but it is not certain that metals were unknown in any period of British history, and it may be that the material used would depend much on the means of the individual; while the chieftain could procure the bronze sword or spear, or even the more valuable iron weapon, his humble follower may have been unable to obtain any better than one of wood or stone. I am inclined to conclude, that the remains we have been discussing belonged to the Celtic race, such a swere in possession of the island when Cæsar invaded Britain. Of any preceding race we have no evidence in Northumberland. No kumbocephalic skull has ever been discovered. All that I have seen or heard of were of the brachycephalic form, which I regard as the true type of the Northumbrian Celts. These few expositions may, I hope, give some idea of what has been done in the investigation of early antiquities in Northumberland; and I must say, that if we are to make any real progress, archæologists must carefully gather facts by actual labour and excavations among the interesting remains which abound in this county, and from these we may eventually be able, by inductive reasoning, to obtain a clearer and more extensive knowledge of primeval history.

Antiquities of the Orkneys. By Mr. GEORGE PETRIE. The author stated that no district of similar extent in Scotland possessed so many aboriginal remains as the Orkneys. These remains were

found in almost every island, and new discoveries were continually occurring. They frequently appeared as green tumuli of various sizes, and sometimes were unexpectedly met with beneath the surface and without any external indication of their existence. The ante-Norwegian antiquities of Orkney might be classified as—1st, Dwellings and other Buildings of Primitive Architecture; 2nd, the so-called Picts'-houses or Pights'-houses; 3rd, Barrows or grave-mounds and ancient graves unconnected with barrows; 4th, Miscellaneous antiquities, such as standing stones and cromlechs. The paper gave a general description of the first two of these classes. The dwellings were subterranean chambers or cells, and brochs or circular towers; while the character of the so-called Pict's-houses the author believed to be sepulchral, and if so they might probably belong to the same race who erected the brochs, and it might be the standing stones also. The author exhibited various ground plans and sections of the ancient buildings, and concluded with a hope that future researches would throw more light on so interesting a subject.

On the Recent Discovery of Lacustrian Human Habitations in Wigtonshire. By Lord Lovaine. Dowalton Loch, in which the structures about to be described were discovered, is a sheet of water of very irregular form, about two miles long and half a mile broad, situated in Wigtonshire, on the west coast of Scotland, at the end of a narrow valley five miles in extent, the whole of which is occupied by a moss, part of whose waters flow into the loch, and the remainder into the sea near Monreith; the elevation of the water-shed near the middle of the valley being almost imperceptible. Sir William Maxwell, of Monreith, has effected the drainage of this loch at his own heavy expense, to the great benefit of his neighbours as well as himself, by a cutting at its southern extremity of no less than twenty-five feet deep, for a considerable distance through the wall of whinstone and slate that closes the valley. The water having been partially drawn off, the bed of the loch exhibits the appearance of an immense sheet of mud, surrounded by beaches of different elevations, covered with large rolled stones and angular blocks of slate. It contains a few small islets, composed apparently of the same materials as the beaches. Sir W. Maxwell, having heard that a bronze vessel had been found in the mud near the southern shore, succeeded in obtaining it, but could not trace other articles of the same description reported to have been found near it. On visiting the spot, 19th August, 1863, to obtain further information, I observed some timbers standing

on an island near the centre of the loch, and was told that some one had been there in a boat when it first appeared above water, and had found bones, a small granite quern, and piles, and a spot was pointed out to me at the extremity of one of the little promontories where similar piles were observable, which, on inspection, I found to be true. These piles varied from a foot to eighteen inches in circumference. Sir W. Maxwell's bailiff, Mr. Chalmers, who displayed great zeal and intelligence throughout these researches, having proceeded to the spot to secure labourers for the next day's search, reported that, though it was not possible to reach the larger island, a smaller one was accessible, and that a canoe lay near it. On reaching the island over about forty yards of mud, I found it nearly circular, about thirty-eight yards in circumference, and thirteen in diameter. It was elevated about five feet and a half above the mud, and on each side of it were two patches of stone, nearly touching it. On the north side of it lay a canoe of oak, between the two patches, and surrounded by piles, the heads just appearing above the surface of the mud; it was twenty-four feet long, four feet two inches broad in the middle, and seven inches deep, the thickness of the bottom being two inches. On removing the stones which covered the surface, several teeth, apparently of swine and oxen, were found; and I proceeded to cut a trench round the islet, and upon coming to the southern end, a small quantity of ashes were turned up, in which were teeth and burnt bones, a piece of a fine earthenware armlet of a yellow colour, and a large broken earthenware bead, striped blue and white, together with a small metal ornament, apparently gilt; two other pieces of an armlet of the same material, one striped with blue and white, were also found on the surface. On cutting deeper into the structure (the foregoing objects having been found on the outside about two feet from the top), it proved to be wholly artificial, resting on the soft bottom of the loch: the uppermost layer was a mass of brushwood about two feet thick; beneath it large branches and stems of small trees, mostly hazel and birch, mingled with large stones, evidently added to compress the mass; below that were layers of heather and brushwood, intermingled with stones and soil, the whole resting upon a bed of fern about a foot thick, which appeared in all the structures examined to form the foundation. The whole mass was pinned together by piles and stakes of oak and willow, some of them driven two feet and a half into the bottom of the loch, similar to those above mentioned. The islet was surrounded by an immense number of these, extending to a distance of twenty yards around it; and the

masses of stone, which were apparently meant to act as breakwaters, were laid amongst them. The one next examined stood about sixty yards off, at the extremity of a rocky projection into the loch, but separated from it by the now hardened mud. It was smaller, and the layers were not so distinctly marked, and some of the timbers inserted in it under the first layer of brushwood were larger, and either split or cut to a face. A stake with two holes bored in it about the size of a finger, a thin piece of wood in which mortices had been cut, and a sort of box, the interior of which was about six inches cube, with a ledge to receive the cover, very rudely cut out of wood, were found. I succeeded two days afterwards in reaching the largest islet in a boat. It appeared by measurement to be three feet below the level of the other islets; but it was much larger, and several depressions on its surface showed that it had sunk. Wherever the soil was not covered with stones and silt, teeth were scattered all over it. We found quantities of bones at different depths in the mass, but always below the upper layer of faggots, and towards the outside. The progress of the excavation was very soon stopped by the oozing in of the water; but a workman, plunging his arm up to the shoulder into the soft material, brought up handfuls of the fern layer, mingled with sticks and hazel nuts, and large bones, believed to be those of oxen. Near the spot lumps of sand and stone fused together were picked up. On the south side of the island extraordinary pains had been taken to secure the structure; heavy slabs of oak, five feet long, two feet wide, and two inches thick, were laid one upon another in a sloping direction, bolted together by stakes inserted in mortices eight inches by ten inches in size, and connected by squared pieces of timber three feet eight inches in length. It extended to the length of twenty-three yards, and its base, about five yards beyond the surface of the mud, was formed of stems of trees laid horizontally, and secured by stakes. In other respects the formation resembled that of the other islet, but it was far larger, measuring one hundred yards round by about thirty-six yards across. No building of any sort was discovered, but a large plank of oak, twelve feet long, fourteen inches broad, and seven inches thick, lay covered with stones on the north side. The sinking of the mud had by this time laid bare a second canoe between the islet first examined and the shore; it was eighteen feet and a half long, two feet seven inches wide, and barely two inches deep: a block of wood, cut to fit a hole left probably by a rotten branch, was inserted in the side, two feet long, seven inches wide, and five and a half inches thick, and had there been secured by pegs driven through the side; across

the stern was cut a deep groove to admit a backboard. A hole two inches in diameter was bored at about one-third of the length of both canoes in the bottom. This was so rotten that it would not bear my weight without breaking. The next day, being unable to reach the last mentioned island, I found upon the spot which had been indicated to me on my first inquiry no less than six structures, similar to those before described, in a semicircle. They were, however, much smaller, apparently single dwellings. Though upon some of them charred wood was found, nothing else was discovered except a morticed piece of timber, which might have drifted there; and in one, inserted under the upper layer of brushwood, a large oak timber, measuring eight feet long by three feet in circumference. Throughout these investigations no tool or weapon of any sort has come to light. In the layers the leaves and nuts were perfectly fresh and distinct, and the bark was as plainly distinguishable on the stems and timber as on the day they were laid down, as were also the heather and the fern. It is difficult to conjecture the state of the loch when these edifices were formed, and whether or not they were completed at one period. The finding of the large stones in the layer of ferns might lead to the belief that they were gradually raised as the waters of the loch increased, and the necessity of strengthening them by breakwaters would seem to prove that the loch must have risen considerably before they were abandoned. No other sort of building has been discovered on them; but the great number of teeth scattered over the surface of the larger island, and even on the mud surrounding, and the immense expenditure of labour indicated in the shaping and hewing of the large timber with tools, which must have been from the work produced of the rudest description, betoken apparently a considerable population. The loch must have remained for a considerable period at each of the different levels before mentioned; at one time six or seven feet above its last level (that is, before its drainage was effected), to which it was reduced by three cuts made to feed neighbouring mills, one certainly of great antiquity. At three feet and a half below the ordinary level there are unmistakable appearances of a former beach, with which the top of the first mentioned islet almost exactly coincides. It is remarkable that, though there are many rocky eminences in the bed of the loch, none bear token of ever having been used for the erection of these dwellings, which seem to have invariably been based upon the soft bottom of the loch, where the intervening mud and water may have afforded the inhabitants a greater security from attacks from the shore. I had not time to examine fully the shores of the

loch, but I was assured by Mr. Chalmers that he had examined them carefully without finding traces of other structures. On a hill to the south there are remains of a Danish fort (*i. e.* a circular entrenchment); and the very ancient ruin called Long Castle is on an adjacent promontory on the north side.

Since writing the above, a very old man in Sir William Maxwell's service told me that in clearing out a channel between a small wooded island in Myston Loch, close to Monreith House, and the beach, he remembers there being found layers of timbers, piles, and flat stones laid in circles. I have also obtained from a farmer living near Ravenstone Moss a paddle of black oak, three feet long, fourteen inches broad, and one inch thick, which with four or five others he had found in that moss, lying close to a mass of timbers about six feet from the surface; this I have every reason to believe formed part of a structure similar to those described. I should have mentioned that, though retaining its shape, the timber is for the most part completely decayed, except where it has been protected from the action of the mud.

Dowalton Loch lies one mile to the left of the highroad, half-way between Wigton and Port William. The name of the loch is probably derived from the MacDowals, formerly lords of this part of the country, and possibly of Irish origin, constant communications with the north of Ireland having taken place from the earliest period. Sir William Maxwell suggests as an easy explanation of the different levels found in the loch, that the waters originally discharged themselves into the sea from the western end of the valley, a portion of them only now finding an exit that way, in consequence of the formation of the moss towards the centre of the valley, which compelled the remainder to flow into the loch. In this case the structures must be supposed to be formed in the early stages of the growth of the moss, whilst the loch was so shallow as to make it easy to raise the moss above its waters, and yet deep enough to float canoes, and afford the desired security from an enemy,

Professor Wilson said that a good deal of attention had been paid to this subject by Scottish antiquarians; and Mr. Joseph Robinson of Edinburgh had collected a great deal of important information, showing that a large number of lacustrian structures existed in that part of Great Britain. In a communication from Kincardineshire, addressed to the Earl of Buchan during the time he was president of the Society of Antiquaries, there was a description of a lake in which there were lacustrian habitations. There was also a description of numerous bodies of animals, and of various bone ornaments. Two of

the latter are still preserved in the Museum of Antiquaries, one of them being a plain circular disc, and the other an elaborate interlaced ornament. Similar ornaments were found in early British graves. He was justified in saying that the subject was a very interesting one, particularly at the present time. The opinion he had formed in relation to the discovery of bronze and of metals generally, was that they belonged to a much earlier period than antiquarians had hitherto been disposed to assign to them. The discovery of flint instruments in Suffolk, and the more remarkable discovery in Gray's Inn Lane, London, were altogether free from the supposed difficulties or doubts of modern speculation; and, therefore, were of special importance in the present stage of the argument.

Sir CHARLES LYELL said he thought it was perfectly clear, from the paper which had just been read, that there must have been several successive changes of level in the lake referred to, and he should have been glad to have heard from the author, and also from Professor Wilson, what, in their opinion, is its probable antiquity. The alteration in the levels would account for the changes spoken of by the author. Lord Lovaine had suggested that the changes of level had been brought about by the growth of peat impeding the ancient outlet of the lake. Now, if the archæologist could determine a proximate date to the lowest of these dwellings, and to the ornaments that were found there, it would throw light on one of the most interesting questions in chronology. It would throw light on the rate of the growth of peat, one of the modes of measuring the chronology of what geologists considered very modern periods—modern, that is, in reference to the existence of man; for those lake dwellings, as far as we knew, all of them relate to a period when the form of Europe was just what it is now, or what it was when the Romans conquered Gaul. Contrasted, therefore, with the period of certain animals found in particular formations, these lake habitations were all modern affairs; and if the bronze period could be carried back, as Professor Wilson had remarked, to ages far more remote than had previously been thought, those lake dwellings which exclusively belonged to the stone period, but which also strictly belonged to the period of the living groups, and were long posterior to the time of the extinct animals, must be proportionately ancient, contrasted with historical times. He saw a letter the other day from an able Swiss writer, in which it was stated that not less than one hundred and sixty lake dwellings had been found on the lakes of Switzerland. A large proportion of these lakes had been examined, and it was perfectly clear that some of them belonged to the stone period, without the slightest admixture of bronze. Not far from one of these stone period dwellings there might occur one in which there were, perhaps, two thousand instruments, all of bronze, with hardly a mixture of stone. This was a most important fact in connection with the investigation of tumuli, inasmuch as it was said, with great propriety, that the stone may have been employed sometimes by those who could not afford anything better, while those who were more wealthy used weapons of bronze. It was also said that there must have been a gradual passage from one

to the other. But if in some of those lake dwellings—which were geologically recent—there were found some instruments of stone, and in others, at no great distance, instruments of metal, it was perfectly clear that there was no danger of confounding the two—that there was a long period during which the stone implements prevailed, and another in which bronze or metal prevailed, and that in some cases there appeared to be a gradual change in the art of making those instruments. If, therefore, the bronze period could be carried much further than the antiquaries generally supposed, how ancient must those villages be where there was nothing but instruments of stone. And yet both epochs belonged to a period in which there was not found one of those extinct animals of which geologists had found so many unequivocal remains. He might take that opportunity of saying that however convinced he was that there had been a great number of frauds practised, especially in the valley of the Somme, owing to the great demand for specimens, yet he was also perfectly convinced that ninety-nine—certainly more than ninety—out of every hundred which had been submitted to examination were genuine. His faith in the antiquity of the instruments referred to was not shaken by any of the impositions which had come to light. The fraudulent specimens were invariably covered by a matrix, on the removal of which all the signs of age and of use by man were wanting. They wanted the discoloration of surface, and the original black fringe, and the incrustations of crystallised carbonate of lime, which characterised the genuine instruments. They had also other marks of their pretended character, which are easily distinguished. It had been observed in reference to the flints of Abbeville that if the workpeople there could impose on so many English and French scientific men, how could we know that we had not been imposed on before! First of all he would say that there was an essential difference in the character of the heads, but he would also remark upon a piece of evidence on which every person could judge as well as scientific men. Was it possible, that after we had gone on for nearly twenty years finding flints of the ancient type so rarely that only two or three would turn up at Abbeville during the course of a winter's digging—was it possible, or at least likely, that all at once, in three different places several miles apart, and in gravel of a different character, an epidemic, so to speak, should break out of just the particular types that were wanted? That was a consideration which ought to have prevented many persons from believing in the authenticity of all the specimens that had turned up. Referring again to the peat growth question, he might remark that it appeared from an island in a lake, in the county of Cavan, Ireland, that the lake had acquired additional depth in consequence of the growth of peat stopping the outlet. He could not help hoping, therefore, that we should by degrees get such a measure of the possible growth of peat under such situations as would serve, to a certain extent, to help us in speculating on the minimum of time which a growth of thirty feet of peat may have required. He still hoped that, upon examination, there would be found not merely ornaments but implements, and the remains of domestic and wild animals of the

period. If the lakes of this island were searched with anything like the diligence which was shown in Switzerland, we should doubtless discover a great deal of most important information on the subject of these newly-discovered habitations.

On certain Markings on the Horns of Megaceros Hibernicus. By Professor BEETE JUKES, F.R.S. (C.) Two large portions of bone, found at a depth of forty feet in a peat bog near Longford, were indented near the middle with depressions, arising, in the author's opinion, from pressure exerted from above while they lay at right angles, one upon the other.

Dr. FALCONER said that he had arrived at a diametrically opposite conclusion. Bones of reindeer, cut in precisely a similar manner, had been found in various bone-caves, the *rationale* of the markings being that the strong extensor tendon had been removed by sawing it away from the bone, just as the Esquimaux do at the present day. The "cross-hatching" marks, often to be seen on such mammalian bones, were undoubtedly produced by human weapons. Natural pressure would not have removed the strong outer layer of the bone, and preserved the weak, cancellated interior.

Professors Rupert Jones, Tyndall, and Wyville Thomson concurred with Dr. Falconer, as to the artificial character of the indented cuts—Sir W. Armstrong and Mr. Sorby taking the opposite side of the question.

*Notes on Sir C. Lyell's "Antiquity of Man."** By Mr. JOHN CRAWFURD.

Sir R. MURCHISON said that he had no doubt that the paper which had just been read would create a great deal of discussion. On one point, at least, the author had succeeded in showing that Sir Charles Lyell had made a little slip in the recent work which had been published by him—namely, in attributing to all languages no greater antiquity than a thousand years. Upon the whole, he congratulated Mr. Crawfurd upon the manner in which he had treated the subject; and being an unbeliever himself in the doctrine of the transmutation of species, he cordially approved of the general tenor of the paper.

The Rev. Dr. HINCKS said that he differed so entirely from the author of the paper, that he would not have it supposed that if he did not say anything against some portions of the paper, they were to conclude that he agreed with them. The paper was in fact a complete *omnium gatherum* on every conceivable subject, and it was wasting the time of the section to discuss such a paper. The part he should especially speak on was where he could see very clearly that Mr. Crawfurd had written the most entire nonsense. He pretended to criticise Max Müller, but it was clear that he did not in the least understand what Müller's theory was. He thought it a great pity that Mr. Crawfurd should meddle with such subjects, because it was very evident that he was quite ignorant of the science of language.

* This paper was read before the Ethnological Society in April last. See *Anthropological Review*, p. 60.

After dwelling on matters of detail, and exposing what he thought to be the fallacy of Mr. Crawfurd's assertions and reasoning, he concluded by urging the importance of having a separate section, in which there really could be scientific discussion, especially for questions connected with the science of language.

The Rev. JAMES BRODIE, of Monimail, said, that after having carefully examined Sir Charles Lyell's work on the Antiquity of Man, he had come to the conclusion that he (Sir Charles) had utterly failed to prove that man had existed more than four thousand five hundred years. If there had been time and opportunity he would willingly have stated his reason for coming to that conclusion. As to the origin of variety in the human species, it was very evident, when the mental and bodily characteristics of man were taken into account, that there was but one species. It was very difficult to determine how and when the different varieties came to be formed from the parent stock. The Egyptian mummies and the pictures upon ancient temples proved that distinct races had existed for a great length of time without physical alteration.

Sir GEO. DENNIS said that, like the previous speaker, he also had carefully read Sir Charles Lyell's work, but so far from differing, he cordially agreed with the conclusions at which that learned author had arrived.

The Rev. J. D. GEDEN, of Manchester, said that the whole structure of the Indo-European languages, throughout their geographical area, was substantially the same. The syllables were constructed on the same principle, and the words were subject to the same laws of accident and derivation.

Sir WALTER JAMES said, that while he admired the ability and skill displayed by Mr. Crawfurd in the preparation of his essay, he totally differed from him on some important points. Mr. Crawfurd's hypothesis assumed that the first man, or the first set of men, must have been savages—a theory quite inconsistent with the notion that man was created by a Supreme Being. Arguing *à priori*, the first man created by the Supreme Being could not have been imperfect of his kind, but must have been endowed with strong intellectual and physical powers.

Dr. DONKIN said that Sir John Herschel had clearly shown that where a man was reduced to a point little above the ape in point of intelligence he was physically the most imperfect creature on the face of the globe.

Professor WILSON said that an essential distinction between man and the lower animals lay in the fact that man was what might be called a naturally domesticated animal, whereas the natural state of the lower animals was wild and untamed. Take the wild ox from the plains of India, house and feed him, and give him an artificial existence, and very soon there would be discernible differences of constitution and form distinctly traceable to the new mode of life. In one respect the one would be improved; that is to say, he would be better fitted for the purposes for which man required him, but the animal would, in point of fact, be degraded from his natural position.

But if man were taken from a wild and savage state and domesticated and civilised, everything that was truly noble in him and natural in him would be improved and developed. This proved that man was totally distinct in his primary condition from the lower animals, and that in his first state he must have been—not a half-bred savage, but a thinking, intellectual, noble being. The question of the development of varieties in the human race was one the solution of which would be slow and laborious, and he sincerely hoped that scientific men would not jump to rash conclusions. It was premature to bring forward the ethnological question of the unity of the human race as one capable of receiving a distinct answer. Much light must be thrown on the relations of languages and other subjects before science could solve the problem. In the meantime scientific men must be content to wait and to work backwards from point to point in their investigations—from known to unknown languages and races. One illustration in point could be easily produced, and it was a very valuable one, as showing the development of a new variety of men. When the Anglo-Saxon passed over to the continent of America with the Pilgrim Fathers they were distinguished by all the characteristics of Englishmen. Two hundred years had passed away, and what with the influence of climate, food, and perhaps the admixture of Indian blood, the American race had grown out of the old stock. One could hardly see an American in the street without knowing him to be such. It was important that those who rashly challenged the doctrine of Sir Charles Lyell as to the antiquity of man should bear this fact in mind, for if two hundred years had been sufficient to develope a New Englander, one could easily imagine that the thousands upon thousands of years Sir Charles was prepared to assign to man's past existence were sufficient to change either a white man into a negro or a negro into a white man.

Mr. CRAWFURD, in reply, stated that the exceptional instances of new breeds quoted by Mr. Brodie from the animal kingdom proved nothing in relation to the varieties of the human race. He could point to equally remarkable cases in the human family. Many years ago he met in Burmah a man covered all over with hair, and having no teeth. This man married a fair woman and had two children, one of whom, a boy, was fair, and the other, a girl, was, like her father, covered all over with hair and had no teeth. When the girl grew up, some friends of his were anxious that she should marry, and accordingly they offered a large premium to any one who would take her for a wife. At length a man was found who was sufficiently courageous to do so. Two children were born of the marriage, and again one was fair and the other exactly the counterpart of its mother. But such exceptional facts could not account for the immense varieties in the human species. As to the Americans being a distinct race, there were probably a dozen or more American gentlemen in the room, and he defied Professor Wilson to point them out.

Sir RODERICK MURCHISON congratulated Mr. Crawfurd on having elicited so interesting a discussion. As a geologist, he was impressed with that portion of Sir C. Lyell's work which had reference to the weapons used by the aborigines of France and England. The

character of the formation in which they were found proved clearly that they must have been deposited there ages before the period usually assigned for the creation of man. The application which Professor Wilson had made of that fact was a very important one, for if ethnologists had so much longer a period in which to carry on their researches, they might find means to account for the great changes that have taken place in some portions of the human family.

The Antiquity of Man. By Professor PHILLIPS (C.)—He said that one of the remarkable fruits of Geological investigation was to invest almost every point on the earth's surface with a new interest. The small French village of St. Acheul had long been remarkable for the school of the Jesuits established there; but antiquaries had discovered that it was near a burial ground of great antiquity. In the course of excavation there were discovered the graves of people far more ancient than any known to have been buried there. Other memorials were also discovered; and on one he had obtained from the workmen he read the name of Constantius. A stone coffin was found, and also an armlet, which had been placed on the arm of a buried person. When they looked in front of the great face of excavation, and saw overhead the Jesuit College, the ancient cemetery, and the Roman and Pre-Roman graves, the question arose, "What could be the antiquity of the sand and gravel deposit at the lower level?" In Sir Charles Lyell's recently published volume the situation was fully described. Concerning the deposits, there was no difference of opinion; they were to be reckoned among the later deposits of the geological time, and in the lower parts of these deposits a great number of interesting implements had been obtained, and some of these he exhibited. He described the deposits in detail, from illustrations, stating that fresh water and land shells were found in sand and scattered flints in an argillaceous deposit over it. For the fresh water and land shells in the gravel it was not necessary to appeal to the action of the seas, which, however, was seen in the lower part of the level. There were, in different levels, cases of great agitation of water, comparative agitation, and comparative tranquillity. They might imagine a lacustrine deposit, against which there would be the objection that it would not produce gravel in such a form, it being twisted about in all ways. There ought to have been found lying parallel to the surface of the lake a great number of lacustrine shells; but that was not the case, and the explanation would not apply to the mixture of fresh water and land and amphibious shells. The more ordinary explanation was to suppose the action of a river which had changed its position, so that the flint

instruments found near the bottom might formerly have existed near the top. The arrangement of the sands was obviously of such a kind that they floated over the pebbles, and covered all below. The whole question came finally to this:—Could they determine the age of the gravel beds? They could not escape the conviction that the flint instruments were of the same age as the gravel beds. Upon the supposition of strata having been deposited by river action, the upper surface of the deposits would continually tend to become level, and would be so when the deposits were of an argillaceous nature. In this case the slope varied from $2\frac{1}{2}$ to $1\frac{1}{2}$ degrees. In order to account for the present condition of things, it would be necessary to suppose that the country had been disturbed, and that there had been an elevation affecting the valley of the Somme. On an examination of the locality they would speedily arrive at the impression that it was requisite to remember that there was no period of geological history from which it was safe to exclude a movement of the earth's crust. The map of France showed the causes of the elevation. The rivers ran in parallel lines across the chalk, and it was impossible to separate the circumstance from the similar fact in this country, where these phenomena had been discovered. As there was reason to think that the valley had been subject to upheaval, accepting the supposition, they would not be able to determine the question of age by the excavation of the river. If they followed the suggestion of Sir C. Lyell, and took their measure from Scandinavia, they might come to some determination as to time; but this was a case of a local disturbance of the earth's crust, affecting certain lines of country in a given direction, and apparently ceasing beyond that. As it would be to some purpose to ascertain the antiquity of these deposits, he trusted Sir C. Lyell would not think it otherwise than a compliment to hear an opinion differing from his own.

Mr. WARINGTON SMYTH said that it was only during the last few years that this series had engaged attention In the main facts, as they might be taken by the public, geologists were pretty well agreed; but, nevertheless, the results to be deduced were so momentous in regard to the history of man, that they must be obliged to gentlemen who devoted not only days, but even years, to the elaboration of the details. Professor Phillips differed in no great degree as to his facts from Sir C. Lyell; but as to the explanation of these phenomena and the physical agencies by which they had been produced there were differences of opinion, some attributing the present position of these curious strata to the erosive action of water, and some to elevations which we knew from other sources the whole of these countries had been subjected to within a recent period.

On the Alluvial Accumulation in the Valley of the Somme and Ouse. By Mr. R. A. C. GODWIN-AUSTEN, F.R.S.—The object of this paper was to show that these two river valleys belonged to areas over which the geological changes had differed so greatly that, at present, comparisons could not be made; that the materials of the gravel-beds of the Ouse had, like those of all the rivers of the east of England, been derived from the "boulder formation;" and that the state of the animal remains indicated that they belonged to the fauna of the period antecedent to the boulder clay; consequently that, should it be proved that flint implements were to be met with in the Bedford gravel-beds, it would not prove that the *Elephas primigenius* and its associates were contemporaneous with man. The valley of the Somme was shown to belong to an area which lay beyond the "boulder formation"—that the series of alluvial beds differed greatly in respect of the physical conditions under which they had originated, yet that they indicated a definite order of succession, and implied a vast lapse of past time; in each of these flint implements have been said to have been found. The only evidence on this point which the author considers to be reliable is that with respect to the Champ de Mars, near Abbeville, where the beds belonged to the most recent portion of the alluvial series of the Somme, in the "subaërial" accumulations. The author further attempted to show that there is no sufficient evidence of a post-glacial elephantine period; and also that the Somme valley could never have been the line of drainage of a vast river, but that the phenomena of river alluvia at great elevations are to be accounted for by physical changes of definite date.

Sir C. LYELL said he had expected to hear Professor Phillips and Mr. Godwin-Austen express a wider divergence from his own conclusions than they had done. He took it for granted that Professor Phillips agreed with him in the important point that not only the flint implements which he mentioned in the case of St. Acheul were of the same age as the old river gravel, but also the extinct mammalia. It therefore appeared that they agreed in the important point of the coexistence of man with those extinct animals. The new view which he had attempted to explain was that the upper valley gravel, some eighty or one hundred feet above the level of the sea, was not now in the position it was when the river flowed there, and formed this extensive deposit of sand and gravel. If he understood the argument, there was such a slope of the gravel covered with loam towards the Somme as there would not be if it was the deposit of a considerable river in its original state; in that case the slope would be the other way, from the river towards the bluffs, as in the case of the Rhine and the Mississippi. He was not prepared to say whether it was possible to calculate on the identity of the present state of that surface

with what it was at the very remote period when it was formed, and since which it must have had so many washes by rain during many thousand years. He was not prepared to say whether they could reason in that manner as a change of position. What he said was, that there was nothing in his speculations on the river gravels hostile to the conclusions which Professor Phillips had proposed of there having been possible local movements, or, at any rate, a considerable movement of that country since the old river flowed. He thought it was almost impossible that that should not be the case. Indeed, when he found two levels of river gravel, one higher and the other lower, it generally appeared to him that that must be in consequence of some great movement, that there must have been probably some stationary period, when great accumulations took place; and that there must have been a period of movement, the waters eroding and cutting away the country, until they settled down at a lower level, and there was a formation of gravel there. This was a most propable thing; but they must bear in mind that though they talked of these appearances at two different levels, there were occasionally intermediate levels and deposits of gravel even higher than St. Acheul. It would be difficult to suppose that it was always strictly at two levels that these gravel beds occurred; but there was a prevalence of them at a higher level and at a lower level, that lower level being necessarily higher than that of the present Somme. He, therefore, had no objection to suppose that, after the country had been for some time in that state at which the gravels and sand were formed, there was some movement or elevation during which the river was able to cut the land down, and then form the inferior or lower level gravels; and it did not appear to him that if that view were adopted it made any very essential difference. Professor Phillips thought it made this difference—that the time would be much shorter if there were such a movement, and certainly it would; but he could hardly conceive any movement would enable the river to destroy so much older strata, as it must have destroyed to produce such reiterated river beds. If Professor Phillips could bring evidence of such a movement it would be a great assistance; but that would not alter at all any views which Mr. Prestwich and himself had arrived at with regard to the manner in which the higher and the lower levels were formed. There were other proofs besides the fresh-water shells, and the absence of marine animals, of the fluviatile origin of the St. Acheul gravels. The gravel in the Somme, the Seine, and their tributaries was composed of rock that belonged to the hydrographical beds of those rivers. In addition, there was the presence of fluviatile shells as well as of land animals. He could receive the views of Mr. Prestwich that these gravels were remains of an old river; and he could admit that there might have been such a movement as Professor Phillips had supposed. Mr. Austen, in speaking of the Bedford section, had endeavoured to do away with the argument in favour of the antiquity of man, by supposing that the remains of extinct lions, rhinoceroses, and other animals, taken out of the gravel, which was about thirty feet above the level of the sea, were derived from an

older gravel. He supposed some preexisting formation, out of which the bones were taken, and then deposited in the present, so that that formation which contained the flint instruments would not be proof of the co-existence of man with those extinct mammalia, and that the mammalia existed before, and were washed out into the beds containing the flint instruments. Such an objection might be made to almost every river bed, because rivers were constantly ploughing up their channels, doing and undoing. Therefore, if any animal remains had sunk in the channel, the chances were that they would be torn out again, and rolled on before they got to their final resting place. It was perfectly true that in some of our valleys, such as the Severn, the old drift containing distinct animals will be undermined, and occasionally bones in a state of integrity will be thrown down into the new river bed. There were such cases, and they were guarded with respect to them; but as a general rule, if they found remains buried in gravel, the inference was they were formed during that long period when that ancient growth was deposited, bed after bed, and sometimes partly destroyed and re-deposited. If a geologist wished to draw a contrary conclusion, he was bound to show, first of all, where was the old formations out of which these extinct bones were derived. To make out his theory he would be bound to show that such a formation was under the drift of that country; which, however, was not the case. Under the circumstances, the hypothesis seemed a violent one, formed to get rid of a violent conclusion, to suppose that these bones had been derived from some other formation that existed in the neighbourhood, without a shadow of evidence of there having been such a one, and with all the existing evidence against it. He hoped the conclusion was one which, on reconsideration, Mr. Austen would not continue to maintain.

The Rev. S. W. KING gave an interesting description of a section of the Norwich crag exposed by a recent fall of rock, and said he thought the name of the rock was too local.

Dr. FALCONER could not accept the views of Mr. Godwin-Austen as to the mammalian remains in the implement-bearing gravels having been derived, like the inorganic materials, from a pre-existing age. No two mammalian faunas could be more unlike than those of the pre- and post-glacial ages. The Miocene Tertiary was marked by an exuberance of pachydermatous animals, and an excessively small development of ruminantia. Then, after a lapse of time so great that 1700 feet of strata had been formed in Europe, the Miocene mastodon dies out, giving place to two elephants and some colossal forms of deer; but still there was a marked absence of bovine animals. Immediately after the glacial submergence, new conditions of the surface set in, river-terraces and valley-gravels were accumulated from the pre-glacial material, but the organic contents of these were not those of the older beaches. All their characteristic types were wanting; instead of *Rhinoceros Etruscus*, *Elephas meridionalis*, and the larger deer, we had *Bos priscus* and *primigenius*, the musk ox, and the reindeer, and these bones often in a perfectly fresh condition—not rubbed, and scratched, and polished by ice-friction, as were the relics of the older time. So fresh and complete were these mam-

malian bones, that from a gravel at Folkestone, in exact parallelism with those of the Somme valley and of the valley of the Ouse at Bedford, he had obtained an entire fore-limb of *Hippopotamus*.

ETHNOLOGY.

Varieties of Man in the Malay Archipelago. By Mr. ALFRED WALLACE, F.R.G.S. In the Malay Archipelago are found two very strongly contrasted races—the Malays, and the Papuans. The former inhabit the great western Islands, Sumatra, Java, Borneo, and Celebes; the latter New Guinea and the adjacent small islands. The typical Malays are of a light brown colour resembling cinnamon or lightly roasted coffee, they have constantly straight black and rather coarse hair, little or no beard, and generally smooth hairless bodies, they are of a low stature, rather strongly made, with short thick feet and small delicate hands. The face is broad, the eyebrows flat, the nose small, well formed, with the nostrils somewhat exposed; the lips broad and well cut, the mouth large but not projecting. In character the Malay is impassive, reserved, and bashful. His feelings of surprise, admiration, or fear are not readily manifested, and he has little appreciation of the sublime or beautiful. He is somewhat taciturn, is deliberate when he speaks; he but seldom laughs, nor does he openly express his gratitude for a favour. He revenges an insult more quickly than an injury. He is honest and trustworthy in many matters, but prides himself upon his capacity of lying. His intellect is but mediocre, he is deficient in the energy necessary to acquire knowledge, and his mind seems incapable of following out any more than the simplest combinations He is quick in acquiring mechanical arts, and therefore makes a good servant for simple routine duties. The Papuan is, in many respects, the opposite of the Malay. In colour he is a deep sooty brown or black, his hair is very peculiar, being harsh, dry, and frizzly, growing in little tufts, which in youth are short and compact, but which in adults often grow out so as to form a compact frizzly mop, nearly a yard in diameter. He is bearded, and his arms, legs, and breast are more or less hairy. The Papuan is taller than the Malay, and, perhaps, equal to the average of Europeans; the face is elongate, and the hands and feet rather large; the forehead is flat, the brows very prominent, the nose large, long and arched, with the nostrils hidden by the overhanging top. The face has thus a Semitic character, which is perceptible even in the children. The moral characteristics of the Papuan separate him widely from the Malay. He is impulsive and demonstrative in speech and action. His emotions and passions are expressed in shouts and laughter, in

yells and frantic leapings. He is noisy and boisterous in speech and action, both at home and before strangers. Of his intellect less is known, but it seems at least equal, and probably superior, to that of the Malay. He has a love of art, decorating his canoe, his house, and almost every domestic article with elaborate carving. It must be granted, therefore, that these two races are most strongly contrasted, and if mankind can be classed at all in distinct varieties, the Malay and the Papuan must certainly be kept separate. Besides these well-marked races are the inhabitants of the intermediate islands of the Moluccas and Timor, which, though differing in some degree from both, may yet, in almost every case, be classed with one or the other of them. The Negritos of the Philippines, and the Semangs of Malacca, differ in most important characters from the Papuan races with which they have hitherto been classed, and must be considered to have Asiatic rather than Polynesian affinities. The recent evidence of the antiquity of man, and his having survived geological changes and the extinction of many species of mammalia, introduces a new element into ethnographical researches, and enables us to speculate more freely on the application and origin of races. Mr. Darwin's researches on the structure and origin of the coral reefs of the Pacific, render it highly probable that great islands, or even continents, have recently sunk beneath its waters. The present distribution of animals in the Pacific Islands leads us to conclude that this subsidence is geologically recent. The inhabitants of all the Pacific islands as far west as New Guinea and Australia, have much in common, while they differ greatly from other races. Combining these facts and boldly following their indications, we may divide the Malay Archipelago by a virtual waving line through the Moluccas, so that all the tribes to the west of the line will be Malayan and of Asiatic origin, and all to the east Papuan or of Polynesian origin. This division is in harmony with that which has been shown to exist in the animal productions of the same regions, and obviates the difficulties attending every theory hitherto proposed as to the affinities and derivation of the Malayan and Polynesian races."

Professor JUKES said he could quite confirm Mr. Wallace's statements as to the distinction between the Malay and the Papuan races. He differed from him, however, in identifying the frizzled hair of the latter with that of the Negro. He was much struck with the latter part of the paper. The author had arrived at conclusions with reference to the antiquity of man, which he (Prof. Jukes) had ventured to draw some twenty years ago, though he had not ventured to state them publicly otherwise than in magazine articles and other anonymous papers. It had been for twenty years impressed upon his mind

that the great depression of land in Oceania was one of the chief causes operating for the distribution of race. If there had been a large continent in that part of the world inhabited by man, which continent has sunk and disappeared, and the tombs of which now exist in the coral islands, then the antiquity of the inhabitants of those islands would have to be dated from very far back indeed. In private discussions among his friends twenty years ago he never attributed to the human race an existence of less than a hundred thousand years. He had no data for arriving at that opinion, but the impression had been produced on his mind, and he still entertained the same conviction. A hundred thousand years was, after all, a small period to allow for the depression of a vast continent and the springing of a number of coral islands out of the bed of the ocean.

Dr. JAMES HUNT said that the paper which had just been read was one of the most important that had been submitted to the notice of the British Association. In the last portion of it the author very properly stated that the modern discovery of the vast antiquity of man had opened up fresh ground, and had, in fact, put the whole science of man in a new light. Some four years ago he had the honour of reading a paper before the Association on the harmony of the evidence in support of the antiquity of man, and on that occasion he spoke of the inadequacy of any of the views which had then been publicly put forward. The fact was, that when we talked of the existence of the human race we got out of our depth, and there were no data on which to build our conclusions. When he read the paper to which he alluded before the Association he quoted a remark from a German work which produced a smile, and which would probably have a similar effect now. The remark was to the effect that man had existed for not less than 35,000 years, and that there was every reason to believe that he had existed for nine millions of years. With reference to the question of race, the author of the paper very properly attached great importance to the principles of art which were found in different races. He entirely agreed with that sentiment. He would ask the author, however, whether he thought that similarity of language was a test of affinity, and whether, in the absence of civilisation, he did not admit that in certain races there was an inability to accept civilisation? Mr. Wallace ascribed the changes that had taken place in the races of Europe and America to physical causes. He would ask whether there was not a mental influence also at work in producing these changes?

After a few remarks from Mr. Crawfurd,

Mr. WALLACE said that the questions which Dr. Hunt had put to him were exceedingly difficult ones. With regard to language, he thought it was inferior as a test of race to physical and moral characters, but it was a very good test of close affinities of races which had been recently separated. It did not appear to him that it could be said of any race of men that it was unable to accept civilization. The inhabitants of Great Britain were once savages, and the Romans might have said of them that they were incapable of receiving civilisation, with as much justice as we could say so of the Negro. As-

suming the correctness of the hypothesis of the remote antiquity of man, it might be argued that if one people—the Britons—could exist 50,000 years uncivilized, why could not another race exist 52,000 years without losing their capacity for improvement. With regard to the influence of mind on the changes of race, there were no doubt many varied causes to be taken into account, and he was not prepared to say that any particular influence had not been at work.

Sir J. RICHARDSON said that the very first problem in reference to the antiquity of man had not yet been solved. As yet we could not venture to state what was the precise age of a bank of peat moss. The paper which had been read seemed to him to strengthen the theory of the unity of the human race. If ethnologists had only time to work out the changes in the human family some very serious difficulties would be removed.

Ethnology of Eastern Mantchuria. By Captain FLEMING. The particulars in this paper will be found in the recent work of this author.

On the Ethnology of Ceylon, referring especially to its Singalese and Tamil inhabitants. By MUTU COOMARA SWAMY. The author commenced by saying that the population of Ceylon was nearly three millions, and that its inhabitants, who were distributed among a great variety of races, might be classified under the heads of European, Asiatic, and Eurasian. The population was not great, and consisted chiefly of English, Irish, and Scotch emigrants, employed in the civil and military service, or on the plantations. The Asiatics of Ceylon are the Veddahs, the Singalese, the Tamils, the Moors, and the Malays. The Veddahs are hunters, and are supposed to be the aborigines of the island. The Tamils of Ceylon belong to the same race as the Tamils of Southern India, and consist either of those who have been on the island for centuries, or who are recent emigrants. They are to be chiefly found in the north-east portion of the island, and their two great capitals are Jaffna and Trincomalee. Their main occupation is agricultural. The coolies are the labourers of the island. They cross over in large numbers from the continent during the coffee season. The Singalese are the inhabitants proper of Ceylon, and range themselves under the heads of Kandians, Low Country Singalese, and Rhodiahs. The Kandians are the inhabitants of the hill country, and are a hardy robust race, never till recently intermingling with their low country brethren. Their language is made up of three component parts—Elu, a Singalese pure, the Pali, and the Sanscrit. They possess an extensive literature, and their religion is Buddism. The low country Singalese are either Bhuddists, Roman Catholics, or Protestants. The influence of Roman Catholicism is very great, and

the people are divided into classes after their occupations. The Malay population of the island is small, and the inhabitants form the Ceylon Rifle Regiment. They are faithful soldiers, brave and obedient; and in their religion thorough Mahommedans. The Moors are the small traders and shopkeepers of the island.

Mr. ELLIOTT said that the Tamil nation was the type one of the two great nations of Southern India, those who spoke the language of the south, and those who spoke the language of the north. Each was totally distinct from the other; the northern dialect being derived from the Sanscrit and the south being Tamil, and having a distinct alphabet of its own. He thought it probable that the slave population had also spoken a distinct language, as there were words still used by them which were not easily recognisable in the Tamil. These were probably vestiges of a tongue which was lost in the extreme state of destitution to which the race was reduced.

Mr. CRAWFURD expressed his gratification that the population of Ceylon had increased so much within the last twenty years. He had not the least doubt, however, that the island could easily support ten millions of inhabitants, and that it would ultimately reach that number if the Government continued to be wisely and liberally administered. He believed that all the pearls used by ladies in this country were Singalese pearls, being distinguishable from the Persian by their peculiar whiteness. The Malays of Ceylon were a very useful and industrious race.

Dr. HINCKS, having made some allusions to the peculiarities of the religion of some portions of the Asiatic population of the island,

MUTU COOMARA SWAMY entered into some further particulars respecting the distinctions observable between the philosophical religion of the Hindoos and Bhuddism. The former taught the doctrine of the absorption of the soul into the Deity and of four degrees of happiness—the existence of the soul in the same sphere as God—its still closer affinity—its assumption of the form of God, and finally its absorption into the Deity. Three-fourths of the religion of the Hindoos was philosophical, and he claimed for his countrymen that they had worked out metaphysical problems of the same nature as those of Kant and other German philosophers, long before Kant's philosophy was thought of in Europe. It was, however, a failing in his philosophical countrymen that they often found themselves in cloudland, and went so far in their religious speculations as sometimes to doubt even their own existence. In this respect they showed the same fate as other metaphysicians who were apt to lose themselves in the labyrinth of their own subtleties.

Ethnology of the Island of Formosa. By Mr. Consul SWINHOE. This paper was read before the Ethnological Society in the beginning of the year.

On the Origin of the Gypsies. By JOHN CRAWFURD, Esq. This paper was read before the Ethnological Society last session. Mr.

Crawfurd says:—"The origin, as our old English has it, of the 'outlandish persons calling themselves Egyptians or Gypsies,' and constituting 'a strange kind of commonwealth among themselves of wandering imposters and jugglers,' is, at least, a subject of great curiosity, not to say of etymological import. Although their first appearance in Europe be coeval with the century which witnessed the discovery of the New World and the new passage to the Indies, no one thought of ascribing to them a Hindu origin, and this hypothesis, the truth of which I now propose to examine, is but of very recent date. Their Hindu origin was not for a long time even suspected; it has of late years, however, received general credence, and, I think, justly. The arguments for it consist in the physical form of the people, in their language, and in the history of their migration. The evidence yielded by physical form will certainly not prove the gypsies to be of Hindu origin. The Hindus are all more or less black; and assuredly no nation or tribe of Hindus now exists, or is even known to have ever existed, as fair as the gypsies of Europe. It is on language chiefly that we must rely for evidence of the Hindu origin of the gypsies, and even this is neither very full nor satisfactory. The dialects spoken by the different tribes of this people, although agreeing in several words, differ very materially from each other. Besides the genuine Indian words to be found in the language of the gypsies, they all contain a large intermixture of foreign tongues, consisting of words of the languages of the people they dwell or have dwelt amongst,—of Persian, of Arabic, of Turkish, of Greek, of Hungarian, and of various Sclavonian tongues; these being, in some cases,—as, for example, in the Persian,—more numerous than the Hindu words. This is what was to be looked for from four hundred years' residence in Europe, and their sojourn among oriental nations in their necessarily slow journey westward. The Indian words which exist in the language of the gypsies are by no means so numerous as the Latin ones which are found in the Welsh and Armorican, or in the Irish and Gaelic, and there will be found wanting in the Gypsy language classes of words which are indispensable towards proving it of Indian parentage. Of the migration of the Gypsies from India there is assuredly no record in Indian history, neither have we of their arrival in any Asiatic country before they reached Europe. In both France and Italy their first appearance was in an inland city, in both of which they began at once to tell fortunes; a fact which supposes, of course, some acquaintance with the language of the people whose fortunes they pretended to predict. From these two facts, it may be inferred that the Gypsies

were in France and Italy for some time before their appearance in Paris and Bologna. Most probably they came to Italy from Wallachia, through Servia, Bosnia, and Dalmatia, crossing the Adriatic; but what internal commotion led to their adventure is unknown. From Italy, where they were seen five years before they reached France, they probably found their way into the latter country. If the Gypsies were originally an Indian people (and there is no other evidence of their having been so than a few words of an Indian language), they were most probably captives, carried off by some western invader with the hope of peopling his own desert lands. I must come to the conclusion that the Gypsies, when above four centuries ago they first appeared in Western Europe, were already composed of a mixture of many different races, and that the present Gypsies are still more mongrel. In the Asiatic portion of their lineage there is probably a small infusion of Hindu blood; but this, I think, is the utmost that can be predicted of their Indian pedigree. Strictly speaking, they are not more Hindus in lineage than they are Persians, Turks, Wallachians, or Europeans; for they are a mixture of all of these, and in that in proportions impossible to be ascertained."

The Celtic languages in reference to the question of race. By Mr. JOHN CRAWFURD. There exist two living European languages which, going under the name of Celtic, are usually believed to be one tongue, or at least, sister languages of one origin, and spoken by the same race of men. These are, on one hand, the native language of Ireland and of the mountainous part of Scotland, which are beyond doubt essentially the same, and the native language of Wales and Brittany —which are equally sister tongues. I have long been of opinion that the two languages in question are really different and distinct tongues. The words which seem to me most distinctly to prove languages to be cognate are prepositions, auxiliary verbs and conjunctions, adverbs of time and place, those parts of speech, in fact, which form the link of language, and without which sentences cannot be constructed. When these are essentially the same in any two languages, these languages may be pronounced at once as sister tongues, while, when they differ, they may with equal confidence be pronounced as different tongues, or of different origin, although they may contain words in common. Tried by the test which I have endeavoured to describe, the Gaelic and Welsh languages will be found to be, not sister tongues derived from the same parent, as are Italian and French, but two distinct languages. Their particles and auxiliaries are wholly different. The

phonetic character of the two languages differs very materially, and, with the exception of a comparatively small number, their words are wholly different. I have compared, with all the care I could command, the Irish Dictionary of O'Reilly, with the Welsh of Spurrel. The first contains better than 50,000 words, and the last above 33,000; and, in this multitude, I could discover not more than 200 which were common to the two languages. In nearly every case of these there was a difference in the form of the words in the two languages, and this independent of the factitious difference arising from disagreement in their orthographic systems. If the facts and arguments adduced in the course of this paper are admitted, we must come to the conclusion that the Gaelic of Ireland and Scotland, with the dialect of the Isle of Man, on one hand, are the same language, while the Welsh and Breton, with the now extinct Cornish, are essentially the same on the other, the two classes of languages being essentially separate and distinct. So far, then, as language can be considered a test of race, and to the extent that one European race of man differs from another, the parties speaking the two languages must be viewed as distinct original races. The difference between the two peoples in intellectual endowment may not be appreciable, any more than it is in other European races; but, physically, I think it is admitted that the Welsh are shorter in stature and darker in complexion than the people at least of the western part of Ireland, where there has been the least admixture of foreign blood.

On Celtic Languages in reference to the question of Race. By RICHARD STEPHEN CHARNOCK, F.S.A., F.R.G.S., F.A.S.L. At a late meeting of the Ethnological Society of London, John Crawfurd, Esq., F.R.S., read a paper on the "So-called Celtic Languages in reference to the question of Race"; which paper has since been printed by the author. The paper is so badly arranged, that it would be impossible to criticise it as a whole; I therefore propose to deal with it paragraph by paragraph.

The design of the essay is to show that the Gaelic and Welsh are two distinct languages, and are not derived from a common stock. "I have long been of opinion", says Mr. Crawfurd, "that the two languages in question are really different and distinct tongues; and having made such inquiries as were in my power, with the view of determining the question, I propose to state the result in the present paper. The qualifications which I bring to this task are soon told. One of the two languages, the Gaelic, was the language of my childhood (I still retain some colloquial acquaintance with it); and of the

languages of some oriental nations, probably in as advanced a state when their tongues took their present shape as were the Welsh and Irish when theirs did so." This sentence is not wholly undecipherable; but it might have been a little clearer. "In order to determine the consanguinity of languages, the first thing necessary is to find a test by which consanguinity can be certainly ascertained." The following is the author's test. "When between two or more languages there is a substantial agreement in phonetic character, in grammatical structure, and in the great body of their words, such languages may confidently be pronounced to be cognate tongues, or languages having a common parentage." No doubt; but are we to understand that it cannot also be the case unless all these circumstances intervene? In the very next sentence Mr. Crawfurd seems to contradict himself; or, at all events, to lay down a very different proposition. He says, "the words which seem to me most distinctly to prove languages to be cognate are prepositions, auxiliary verbs, and conjunctions, adverbs of time and place—those parts of speech, in fact, which form the links of language, and without which sentences cannot be constructed. When these are essentially the same in any two languages, these languages may be pronounced at once as sister tongues; while, when they differ, they may with equal confidence be pronounced as different tongues, or of different origin, although they may contain many words in common." We are told that the languages of Southern Europe all contain a considerable admixture of Teutonic words, but that they are written easily in words derived from Latin without their assistance, while it is impossible to construct a single sentence of them with words purely Teutonic. When our author speaks of the languages of Southern Europe, I take it he refers to the Italian, Spanish, Portuguese, and Romance languages. If so, after a careful comparison of these languages with the Teutonic, I am inclined to think that Mr. Crawfurd has made use of the word *considerable* for *inconsiderable*. "The proportion of Norman-French in our vocabulary is usually reckoned at one-sixth part, or five-sixths of our language is of German origin, although in use, from the nature of the words of the latter, the proportion is much greater." Whence did Mr. Crawfurd obtain this information? If he means to apply it to what is now called English, he must have consulted some old work on the language. In the first instance, the author of the paper is wrong in making use of the term German. But I will not quibble with words, as doubtless Anglo-Saxon is intended. It is necessary to notice this, because not only is there a great difference between these two lan-

guages, but the Anglo-Saxon (at all events, in proportion to the words that it possesses) contains a much larger number of words of Latin origin than do the German languages. I am aware that Hickes maintained that nine-tenths of the English dictionary was of Saxon origin, because there were only three words of Latin origin in the Lord's Prayer; that Sharon Turner was of opinion that the relation of Norman to Saxon was as four to six; and that another writer, who estimates the whole number of English words at 38,000, assigns 23,000 to a Saxon, and 15,000 to a classical source. Thommerel was of opinion that of 43,566 words, 20,853 were of classical, 13,230 of Teutonic (Anglo-Saxon?) origin, and that the remainder were from miscellaneous sources. None of these statements, however, will hold water at the present day. Out of the 80,000 words which now make up our language, considerably more than 30,000 may be traced to the Latin and Greek. As for the Norman element, instead of its constituting, as Mr. Crawfurd states, one-sixth part of the English language, it probably does not constitute one-fiftieth part. After descanting at some length on the Welsh and Gaelic, in order to prove that they are distinct languages, the author of the paper arrives at the question of grammatical structure. "I come next to the question of grammatical structure as a test of the affinity, so much relied upon of late by learned Germans. It is by this they come to the startling conclusion, that the leading languages of ancient and modern Europe have all sprung out of a dead language of India, or yet more extravagantly, from a language of the highest table-land of Central Asia, of which the very name and locality are pure myths. The corollary follows that all the races speaking them—black, brown, and fair, the Celts included—are of Eastern origin;" but, as we have shown elsewhere, it is not a corollary at all. In order to illustrate what is said in the previous passages, with regard to the European languages, Mr. Crawfurd very unreasonably refers to the American languages (1,200 in number), which he says have a common grammatical structure—one which distinguishes them from all the other languages of the world. He says, "that this, adopting the German test of affinity, ought to prove that all the American languages had one common origin, but that the theory is at once demolished by the crushing fact that, with the exception of the languages of a few neighbouring tribes and nations which have borrowed a small number of words from each other, the vocabularies of the numerous languages in question are wholly different; and that an agreement in grammatical structure is, therefore, in this case, no evidence of language; nor does

it even go to prove affinity of race." Is not this something like a *petitio principii?* Must we, as a matter of course, assume that because the vocabularies of two languages are wholly different, that, therefore, such languages cannot have been derived from the same stock? It is not difficult to conceive that a language may lose many of its words, or that words may be replaced by other words; and that, nevertheless, the grammatical structure of the language may remain the same. Missionaries asserted that in the middle of the eighteenth century the American tribe called the Araucaños spoke hardly a word which was not Spanish, though they preserved both the grammar and syntax of their own native speech.* Mr. Crawfurd says, "From the eastern borders of Bengal, to the utmost limits of China, the numerous languages spoken are, without an exception, monosyllabic, or their words consist of single syllables—which necessarily admit neither of inflection nor composition. They are, therefore, unavoidably of the same grammatical structure. But the words of these languages are wholly different, even when the race of man is the same; and of the races of man there are at least two clearly distinct ones, the Chinese being an example of one, and the Birmese and Siamese of the other." This sentence is rather foggy, but what is meant is, first, that in the tract of country referred to there are two distinct races; secondly, that although the structure of all the languages spoken in such tract is the same, the words are wholly different, not only when the race is different, but when it is even the same. In answer to this, I will simply ask, does Mr. Crawfurd mean to assert that the Siamese, or as it is more correctly called the language of Thaï, is a monosyllabic language. If the author of the paper had taken the trouble to look into the matter he would have found that the Siamese is not a monosyllabic language; and, further, that one of the two languages spoken in Birma is a polysyllabic language.†

I may here remark, that race can never to a certainty be determined by language. People of the same race may speak two different languages, while on the other hand people of different races may speak the same language, or at all events languages derived from the same source. The French and Italians are of a different race, although both nations speak a language of the same origin, *i.e.*

* Cf. Herves, Catalogo, t. i, p. 16-23; Max Müller, Lectures on Language, London, 1862, p. 77.

† The languages spoken by the people north of Birma, Siam, Cambodia, and China, are all polysyllabic; whilst in Japan, at the extreme east of the Chinese empire, of the two languages spoken, the *Koyo* is monosyllabic, whilst the *Yomi* is polysyllabic.

derived from the Latin. What would be thought of a man, who, having over night given what he considered cogent reasons for not visiting Rome during the summer months, on account of the malaria, should the next morning start direct for the Holy city; or of another, who, convinced of the inutility of fomentations for gout, should forthwith set to work to bathe his great toe in cold water? But the author of the paper does something like this. After going to the trouble of adducing arguments to prove, *as he thinks*, "that the boasted test of an agreement in the mere structural form of language is inadmissible as evidence of affinity;" and having travelled to the Malayan and Philippine Archipelagos and the islands of the Pacific Ocean, to disprove the structural theory advocated by Max Müller and others, Mr. Crawfurd proceeds nevertheless to compare the Gaelic and Welsh, with the view of showing that in point of structure they are entirely different languages. Again, after informing us that the formation of compound words, by the help of prepositions and postpositions, has been stated to be a distinguishing characteristic of all the languages called Indo-Germanic, or Aryan; and among these, as a derivative of the Sanskrit, the Gaelic, and Welsh, the author of the paper has the assurance to tell us "that no such manner of compounding words is known to either of these languages; and, therefore, in so far as this character is concerned, they are not of the pretended class in question." This assertion is wholly inexcusable, for, if Mr. Crawfurd, who "still retains some colloquial acquaintance with the Gaelic," had only examined the dictionaries of the two languages in question for the space of twenty minutes, he would have found that in more than one-third of the words in these languages the first syllable is a prefix. Perhaps, indeed, there is no language in which we have such decisive evidence of the formation of words by prefixes as the Welsh. The principal prefixes in the Welsh are *ad*, *am*, *an*, *ar*, *cyd*, *dad*, *dar*, *de*, and *dy*; *di*, *go*, *gor*, *hy*, *rhag*, *rhy*, *tra*, *try*, and *ym*. The prefix *ad* or *at* is of the same force and signification as *re* in "regenerate," "return;" thus, from *ad* and *galw*, to call, we get *adalw*, to recall. *Am* is both a preposition in the ordinary sense of that word, and a prefix. In composition it answers to the Latin *circum*, and the Greek περι, αμφι; thus from *am* and *chwyl*, a revolution, is *amchwyl*, a circumvolution. *An* or *a* (which, before *l* and *r*, takes the form of *av*,) is used in a privative sense, like the Greek αν or α, and corresponds with the Latin *in*, and the English *in* and *un*; thus from *an* and *mâd*, good, is *anvad*, bad. *Ar*, as a preposition, signifies close to or upon; hence from *ar* and *mor*, the sea,

we get *Amor-ica;* from *ar* and *coel,* a belief, *argoel,* an omen, a sign. The preposition *cyd* (var. *gyd, gyda*) is = the Greek *συν,* the Latin *cum.* As a prefix, it varies according to the letter which immediately follows; and is found as *cy, cyd, cyt, cyf, cyv, cym, cyn,* and *cys.* Thus from *cyd* and *gradd,* grade, station, degree, is *cydradd,* of the same rank; from *cyd* and *tir,* land *cyttir,* land held in common; from *cyd* and *mer,** water, sea, *cymmer,* a confluence of waters; from *cyd* and *dala,* to hold, *cynnhal,* to hold together, support, maintain; from *cyd* and *stâd,* state, *cystadl,* commonly pronounced *cystal,* of the same state. *Dar, de,* and *dy* are used very arbitrarily, and their power cannot be well ascertained; thus from *dar* and *eb,* a saying, is *dareb,* a proverb. *Di* is a privative, = the Latin *de* in *dedecus;* thus from *di* and *fydd,* faith, is *difydd,* faithless. *Go* is cognate with the Saxon *ge;* thus from *go* and *ber,* water, we have *gover,* a small stream. *Gor* is an intensive prefix (although it sometimes serves as a diminutive); thus from *gor* and *gwag,* empty, is *gorwag,* very empty. *Hy* answers to the English termination *able,* or the Latin *osus;* thus from *hy* and *côv,* memory, is *hygov,* memorable. *Rhag* is both a preposition and a prefix of extensive use; thus from *rhag* and *dant,* a tooth, is *rhagddant,* a fore tooth. *Rhy* is an inseparable prefix, which adds intensity and activity to the root; thus from *rhy* and *taer,* bold, is *rhydaer,* too bold, presumptuous. *Tra* is used as a preposition and a prefix, and corresponds with the Latin *trans.* It has also another meaning, corresponding in power with the English *very,* and in form and power with the French *très;* thus from *tra* and *noeth,* night, is *trannoeth,* beyond this night, next morning. *Try* is also used in the same manner as *rhy;* thus from *try* and *llaw,* a hand, is *trylaw,* very handy, dexterous. Lastly, *ym* makes the verbs and verbals to which it is prefixed reflective; thus from *ym, ad,* and *nabod,* is *ymadnabod,* to know oneself.† Mr. Crawfurd considers the glossarial test in a comparison of languages the most complete and satisfactory. Having compared the Irish dictionary of O'Reilly with the Welsh dictionary of Spurrell, he tells us that the first contains more than 50,000 words, and the latter above 33,000, and that he has not been able to discover more than two hundred words common to the two languages. I will not venture to say whether the last part of this sentence is or is not correct; but when a philologist is desirous of comparing one language with another, one would think it would be the most reasonable to consult two dictionaries in which a comparison

* In some words, as will hereafter be seen, the two first syllables are prefixes.

† Cf. Archdeacon Williams's Gomer.

could be made. Now, O'Reilly's is the most voluminous dictionary of the Irish language; whereas Spurrell's is little more than a pocket dictionary; and, if Pughe's Welsh dictionary had been consulted, the number of words would have been found to exceed 95,000. Again, "taking the Welsh vocabulary, the Gaelic words in it will not exceed one word in one hundred and sixty-six. The English language, it is needless to insist, contains an incomparably larger proportion of Latin words, directly or indirectly introduced; the French, Italian, and Spanish languages a much greater proportion of Teutonic words. Even the Spanish at least has many Arabic words. But we know historically the real origin of all these languages—know the English to be of Germanic origin, the languages of the South of Europe to be derived from the Latin; while the other elements of all of them are extrinsic." This is partly erroneous, and partly puerile. The English is not of German origin, but a language, which, if every word be taken into account, is principally based upon Greek and Latin, derived partly through Saxon and Norman French, and partly direct from the two former languages. Mr. Crawfurd says, "With respect to the class of words common to the Gaelic and Welsh, they seem to me to be such as we can readily believe would gain admission into the languages of neighbouring people; and the probability is, that they proceeded from the language of the more advanced and powerful, to that of the least advanced and weakest. Such infusions are well known to have taken place among rude nations in other parts of the world, where intercourse was far more difficult, and the two British islands cannot be supposed to be an exception. The words common to the two indigenous British tongues are of a very miscellaneous character, but they are never such as are indispensable to the structure of language, while both tongues can be written or spoken without their assistance." Mr. Crawfurd then refers to the names of plants and animals, indigenous or of foreign origin, immemorially acclimated or domesticated, with the object of showing that they are generally different in the Gaelic and Welsh. Some stress is laid upon the fact that the Gaelic possesses no specific name for the bull or the entire horse. Mr. Crawfurd does not inform us whether the practice of gelding was common, or even known, to the Celtic nations. The Romans, no doubt, gelded their horses, at least those which they employed for common and domestic purposes. The nations of Africa and Asia, except the Chinese, never geld their horses at all; and some kingdoms of Europe have not yet adopted this barbarous practice. The custom in question belonged more peculiarly to the Scythians and Sarmatians,

than to any other nations; and the Franks first learned the art and custom from the Hungarians, and to this day the French call a gelding "cheval hongre." Mr. Crawfurd says, "the existence of Latin words in any Gaelic writings handed down by tradition, I may take this opportunity of stating, would prove them to be more or less adulterated, if they pretended to an antiquity beyond the era of the introduction of Christianity. Applying this rule to the poems of Ossian, whether those translated or paraphrased by McPherson, or such as have been handed down by oral tradition without his name, we discover words of Latin origin, which, had they been of the ages of Ossian, whose heroes are always represented as heathens, would not have been the case. We find, for example, such words as shield, sword, arms, gold, and silver, of Latin origin; but, above all, the names of the numerals from an unit up to a thousand, a class of words here of a compass not likely to exist in the language of a people so rude as must have been the Irish and Caledonians of the time ascribed to Ossian. That the poems of Ossian are spurious is one thing. There cannot be much doubt about the matter; but that the Latin words in these poems may have found their way into the Gaelic languages prior to the introduction of Christianity, does not seem at all unreasonable, and may be accounted for without any great difficulty. Let us see what our author says on the etymology of local names." He derives Armorica from two words common to the Gaelic and Welsh languages: viz., from *ard*, a height or high, and *muir*, the sea; or more probably, *mor*, great or extensive." But, taking into consideration the peninsular character of this part of France, the common etymology from *ar-mor*, "upon the sea," would seem to be the most reasonable. In like manner, Muirar, the native name of the province of Moray, in Scotland, is doubtless an inverse, from the Gaelic *muir-ar;* whilst Pomerania in Prussia has been very reasonably derived from the Slavonic *po-mor*, "upon the sea." We are told that "the etymology of the word Wales, which the French write Galles, is unknown, unless it be a corruption of the Roman word Galli." It is clear enough that *Wales* and *Galles* are the same word. The Anglo-Saxon has *Walas*, the Welsh, Britons; *Walli*, Britannia; *wealh* (pl. *wealhas*), a foreigner, stranger, one from another country, a Welshman, Welsh. The old German has *Gal*, *Gall*, *Wall*, *Wale*, *Weale*, *Walah*, a stranger, a Gaul, a Roman; *Walcholant*, Gaul; the modern German, *Welscher*, an Italian; *Welschland*, Italy. The Med. Latin *Wallus*, *Gualus*, *Gaul;* all which words are, without doubt, derived, with the aid of the prefixes *g* and *w*, from the old German *al*, *el*, strange, foreign; from

the Latin *alius*. Hence the Alamanni, who gave their name to Allemagne, *i.e.*, Germany, were called. There was also a Gaulish tribe called the Allobroges, who would seem to have derived the first part of their name from the same source.*

Mr. Crawfurd says "of the etymology of the word Britannia, employed by the Romans, there is certainly no certain knowledge. Some have derived it from the Prydain of the Welsh or the Bhreatunn of the Irish, but I think it far more likely that both these words are corruptions of the Latin word Britannia." This is not at all probable. The word Britannia, in Latin, means nothing at all; and is merely the latinized form of the original name of the country. The etymologies of the name Britain are legion; but perhaps the most reasonable is that from *bret inn*, "high island." Mr. Crawfurd is of opinion that the appellations of all great countries have been bestowed by strangers more civilized than their own inhabitants, and that the names of Italy, Spain, and Germany are examples in Europe; and India and China in Asia, to say nothing of the great geographical divisions, Europe, Asia, Africa, America, and Australia. Perhaps the author of the paper is right, although he has not proved it by *all* the examples which he has given. Further, whatever may be the European designation, it will be found to have been formed in many instances from a native word. Mr. Crawfurd concludes, "If the facts and arguments adduced in the course of this paper are valid, the languages which are its subject are two distinct and separate tongues. Bede, indeed, seven centuries ago, pronounced the Welsh and Irish to be as different from each other as Latin and Saxon. So far, then, as language can be considered a test of race, and to the extent that one European race of man differs from another, the parties speaking the two languages must be viewed as distinct original races." It remains to be seen whether by the facts and arguments which have been adduced, Mr. Crawfurd has proved his case.

To conclude: notwithstanding that a large amount of matter has been brought together in a small space, I am disposed to think that Mr. Crawfurd's paper is illogical and inconclusive; and that it is totally unworthy of the author of the very able dissertation on the Malay language.

* Allobroges, pop. Galliæ Narbonesis, sic dictus, quod ex alio agro translatus esset. Vetus Scholiastes ad Sat. 8, Juvenalis: *Allobrogæ Galli sunt. Ideo autem dicti* ALLOBROGÆ, *quoniam* BROGA *Galli agrum dicunt,* ALLA *autem aliud, Dicti igitur, quia ex alio loco fuerant translati.* WACHTER.

ADDENDUM.

Personal Recriminations in Section D. The following discussion, though having no reference to the subject matter of the paper, took place in Section D, after Mr. Carter Blake's paper on "Syndactyly in Man and Apes" had been read.

Professor ROLLESTON said he really had no remarks to make upon the paper which had just been read; but there was one thing that he would wish to lay before the Section, in order that they might see that the Sub-Section of Zoology had not been idle. When he heard that a paper was going to be read upon the hands and feet of apes, it struck him that Section D was not wanting in its duty; and a statement having been made, and having received—from causes which he would not specify—a large amount of circulation, it occurred to him that this paper might have some reference to the difference which that statement alluded to between the foot of the anthropoid ape and the foot of man. He thought that the propositions laid down by a gentleman in whom Newcastle had not realized the proverb that a prophet had no honour in his own country, might be going to be controverted; and under these circumstances, Dr. Embleton, another Northumbrian, and himself had thought it their duty to facilitate the proceedings of this Section by bringing forward the arguments and authorities that could bear upon the subject. They did not consider that the question was one to be settled by rhetoric, and so they set to work, and, as the result of four hours labour, they had prepared a comparison of the foot and hand of the ape with the foot and hand of the human subject. His business for six months in the year was demonstrator of anatomy; and he should be glad if any one who took an interest in the question would call upon him in Subsection D, to act as a demonstrator of anatomy upon this subject, and he had no doubt of being able to remove any doubts that might exist upon it.

The PRESIDENT thought their thanks were due to the President and members of the Sub-Section for their visit, and also for the valuable papers which that department had contributed to the Association. He should be glad, and he had no doubt it would give great satisfaction to all who were present, if Professor Rolleston would at once make the statement to which he had alluded.

Professor ROLLESTON said, if he thought that what he had to say was worth very much, he should not have acceded to that request, because, though not a Newcastle man, he was quite north countryman enough to know that there was nothing like standing by old friends; and, therefore, when he had had anything good to communicate, he had always taken it to Sub-Section D. A statement appearing in a journal of large circulation and popularity must have weight in the proceedings of an assembly like the British Association, which itself possessed popular elements; and every now and then, in such periodicals, statements did find place that astonished men of science, and misled persons who had no claim to that title. A statement of this kind, to which he wished now to refer, asserted that every naturalist

knew that the muscle that bound the great toe was, in the ape, also a flexor or bender of the other toes; whereas, in the human foot, it was a single muscle, the effect of which was illustrated in the pirouette of the dancer. So far from every anatomist knowing this to be the case, every anatomist could contradict it. There was no single work, ancient or modern, upon anatomy, which could give authority for such a statement. Henle, one of the best of the old authorities, so far from saying that the great toe of a human being had only one flexor, represented the muscle as sending out its branches to two other toes also. And every competent anatomist would deny the opinion which the writer of the article in question attributed to them all. There were several in that room who would confirm his words—who, with him, had taken the trouble to examine the question; and they would be happy to show to any one the results at which they had arrived. His fellow-countryman Mr. Church, who had written one of the best papers upon the muscles, was in the room, and no doubt his word would be taken; but, if it would not, no one would question the authority of Dr. Embleton, who was also present and ready to confirm what he (Professor Rolleston) stated. A broad and sweeping statement was made, and the world was informed, *ex cathedra*, in a periodical of great circulation, that every anatomist was acquainted with the fact mentioned. He appealed to those gentlemen whom he had named, and also to Mr. Turner and Dr. Cleland, to say whether the statement was correct.

Mr. TURNER could confirm in every respect what Dr. Rolleston had said. There did exist in the human foot a connection between the flexor muscle of the great toe and the other toes, and that connection was the rule and not the exception.

Dr. JAMES HUNT wished to ask Professor Rolleston whether he had any objection to name the "journal of large circulation" in which the statement occurred. If that were done, they would then be in a better position to discuss the matter.

Professor ROLLESTON scarcely thought that the gentleman who put the question could be ignorant of the name of the journal, but would state at once that it was the *Edinburgh Review*.

Mr. BLAKE, in order that the question might be adequately considered, would suggest that the *Edinburgh Review* of April last should be placed upon the table, and the passage in question read at full length. He thought the references in the way in which it had been mentioned, to an article published in accordance with all literary ethics, were calculated much to complicate the subject by the introduction of personal and irrelevant controversy. The customs which usually governed these literary publications prevented the avowal or the disavowal of the review in question; but he might request that the whole of the passage might be read, and further, that certain of the passages in the same publication upon analogous subjects might be read also.

Dr. E. PERCEVAL WRIGHT wished to say a word, in confirmation of the statements of Dr. Rolleston and Dr. Turner. During the last two winters he had taken every opportunity of examining this point,

and in no one instance was he fortunate enough to discover a single exception to what he was inclined to believe was the normal anatomy of this muscle. He had likewise talked over this subject with his friend Professor Hyrtl, of Vienna, whose opportunities for prosecuting the study of human anatomy were immense. and the exceptions to the ordinary state of things found by him were very rare indeed, so that it would appear that the author of the statement in the review in question had mistaken some popular statement for a scientific though easily acquired fact.

Dr. CLELAND could also confirm, so far as his knowledge went, the statement of Professor Rolleston.

Mr. BLAKE said he should be glad to have the statement shown to him in the article in question, in which *simpliciter* it was declared that the insertion of this muscle in the great toe was unaccompanied by any further diversion of it. All that was there stated was, that the force concentrated upon the great toe.

Mr. CHURCH thought that an allusion had been made to his paper. He might be excused for making an observation. It was stated in the review, in reference to this muscle, that in the foot of the various wild nations, and particularly in climbers of trees, it showed more deflection generally. He had tried to find out the differences which the muscles in the human form showed, and though he had had the advantage of the Bodleian and the Radcliffe libraries, he had been unable to find any anatomical account of the wild races of man, in reference to the foot: and he should be glad to know where such accounts were to be found.

Professor ROLLESTON might perhaps be allowed to hint that the discussion had gone quite far enough. The article contained the words, this "solitary tendon passes along the sole of the foot," &c., and after stating the position it occupied with regard to the great toe, proceeded to compare it with that in the foot of the orang-outang which possessed three tendons, so that there could be no question as to the construction which the words bore. On a question of this kind authorities were better than rhetoric, assertions, or opinions, and to authorities he had therefore confined himself.

Mr. BLAKE believed the words "solitary tendon" as the reviewer had used them were strictly correct. By the assertion that one solitary tendon passed along the sole of the foot he was not aware that by any logical construction was conveyed a denial of the fact that before the muscle sent forth its greatest slips, other divergent slips may not be sent forth, and in no passage had the presence of those divergent slips which Mr. Church and Mr. Turner had pointed out been denied. He must complain that such unfair stress should be laid upon the passage, that its meaning should be thus warped, that the logical signification of the words should be, he hoped unintentionally, misrepresented, and that dust should be thus thrown in the eyes of zoologists, as had been done in the theatre of Albemarle Street. He was certain that his anatomical brethren would give the writer of the passage in question credit for having offered a fair and true explanation of the broad differences between the foot of the ape

and the foot of a human being, and the best explanation he could have given to a general audience. The broad question was left just as it was—the whole force of the long flexor in the man was concentrated upon the great toe; but a different condition prevailed in the apes, the force of the long flexor being in their case divided amongst several toes. He was unprepared for a discussion of the myology of apes arising upon a question of their integument, and the subject seemed to have travelled far beyond the record.

The PRESIDENT (Professor Balfour) observed that the question had reference only to the tendon, and reading the passage in the review in the ordinary sense, they had a solitary tendon in the one case, contrasted with three tendons in the other, so that it seemed Professor Rolleston was justified in the construction he put upon the words.

Dr. EMBLETON said he had at one time entertained the belief that that which Mr. Blake had stated was the general rule; but on referring to old authorities as well as to the more recent works upon the subject, he found that the cases where there was a single tendon only attached to the long flexor were the exceptions only.

Dr. CLELAND said the dispute narrowed itself very much to this:—Mr. Blake found fault with the article being treated unfairly; but the force of the reviewer's arguments appeared to be that whereas in the monkey there were three tendons going to three different toes, in the human being the force was concentrated upon the great toe only. The fact was, that the tendon was connected not merely with the great toe, but with all the other toes also, and exercised its pressure upon one as well as upon the other, so that it could not logically be said that the force of the muscle was concentrated upon the great toe.

Professor ROLLESTON wished, before the discussion closed, to ask whether it was not the duty of the president of a section to call attention to such statements, bearing upon his department, as appeared in journals of scientific pretensions? He had thought it his duty, in the sub-section over which he presided, to draw the attention of those who attended the section to statements that were growing current in the periodical literature of the present day. He had taken pains to make himself master of such subjects as came before the section, and to acquaint himself with the popular as well as scientific literature in reference to them, and in that way he had referred to the article in the *Edinburgh Review*.

The PRESIDENT (Professor Balfour) considered that Professor Rolleston had done quite right in calling attention to this matter. It was a great advantage for subjects of this kind to be brought forward and discussed, and he had no doubt that Mr. Blake would agree with him in that opinion.

Mr. BLAKE admitted that Dr. Cleland had very well put the case, and if the word "concentration" was objected to, he would say the greatest power of the muscle was used on the great toe, or employ any analogous word; there was the greatest portion of the power directed upon the great toe in a human being, and there were also divergent slips the existence of which the reviewer did not deny, while, with respect to apes, the force was diverted to several toes. He could

only express his gratification at the way in which this discussion had been carried on by Dr. Cleland, Mr. Turner, Mr. Church, and Dr. Embleton, and expressed himself entirely in accordance with the anatomical facts, as well as the interpretation of the reviewers' version of these facts which these gentlemen had that day promulgated. He wished also to take that opportunity of expressing the very great pleasure he felt on reading the very able paper on the Chimpanzee by Dr. Embleton; and so long as they had scientific facts of this nature, met, as they always ought to be, in a fair spirit, unaccompanied by garbled versions of writers' statements, he should always consider himself proud to enter the lists with such learned anatomists as Dr. Embleton, Mr. Turner, Dr. Cleland, and Mr. Church.

Dr. DAVY did not see how the gentleman who had just spoken could with any justice assert that garbled versions had been brought before the section, seeing that the passages which Mr. Blake desired to be read were read both by the gentleman himself and by Professor Rolleston. He trusted, therefore, that the offensive expression would be withdrawn.

Mr. BLAKE would willingly withdraw the word, if it was offensive to the section, and say that the construction put upon the article by Professor Rolleston was certainly not that which the writer wished to convey.

Here for the present we must conclude our notice of the proceedings of the Association. We have not reported a paper read by Mr. Richard Lee, on the extinction of races, which is of the less importance, as no discussion took place on his paper, and we understand it will ere long be read before the Anthropological Society of London. We shall then be able to present it to our readers at our next issue. We shall speak of Captain Grant's paper, on noticing the forthcoming work by Messrs. Speke and Grant. We can find nothing in Mr. Craft's paper on "Dahomey" which is worth printing. The object of the paper seemed to be to throw discredit on the account which had been sent to this country by M. Jules Gérard; but it was evident that most of the revolting scenes described by that traveller were admitted to exist by Mr. Craft. He, however, denied that they were so revolting. It is very significant that M. Jules Gérard, in his letter to the Duke of Wellington, should have said so markedly, that he was sorry Captain Burton was not present to confirm what he asserted. Mr. Craft was present with M. Jules Gérard, and it is not a little ominous that this traveller never refers to Mr. Craft as a witness. We are sorry to hear that Sir Roderick Murchison at once accepted Mr. Craft's account as correct, and even went so far as to say that "M. Gérard had probably indulged a little in imagination from, the desire to render the description of his journey as graphic as possible."

We hope this assertion was made in the heat of debate, and not as intentionally throwing discredit on the veracity of M. Jules Gérard, who, being in a foreign country, was unable to reply to the attacks made against him in his absence. Let us have patience and hear both sides of the question, before we condemn a traveller, and a foreigner, in this wholesale manner. In the meantime we would suggest to Sir R. Murchison, and to anthropologists generally, whether they do not think that much of the difference in the accounts of the two witnesses may be explained by the two travellers having different "instincts"? Mr. Craft has a certain amount of African blood in his veins, and this must influence his innate ideas. We can, therefore, readily understand that scenes which would be very horrible to M. Jules Gérard, would not appear in such hideous colours to a Mulatto, like Mr. William Craft. While on the subject of half-breeds, we should mention some interesting episodes respecting Mr. Craft, who was put forward as a "pure Negro," and continually described and spoken of as a "fine specimen of his race." What race? we asked, but could get no reply. In the Journal of the Association he was described as "an African Gentleman." But Dr. Philip Carpenter objected to this, and in a letter to the President said, "Mr. Craft is not an African but an American gentleman, having been born in the Southern States of America." Mr. Craft then cleared up the confusion of ideas as to his race, by saying, "he considered himself an Englishman of African parentage, unfortunately born in America." But this is not quite satisfactory, as Mr. Craft knows kimself that one of his parents is a Euro-American, and the other he has never alleged to be of really pure African blood.

This episode well represents the confusion which exists respecting the terminology of anthropological science, and we hope that the promised Report of the Anthropological Society on this subject will soon be issued, that some of this confusion may be removed. We cannot conclude our report without expressing our thanks to the local newspapers of Newcastle for the admirable manner in which the sectional meetings were reprinted, especially in the *Newcastle Chronicle*.

It was not without regret that we looked in vain for the face of the man who has been the Chief Secretary of Section E for many years—we allude to Dr. Norton Shaw. We presume the President forgot to allude to the loss the Section had sustained in no longer having that intelligent, able, and courteous geographer to act as Secretary. We also noticed that the two other Secretaries who have acted with Dr. Shaw for some years past did not act this year.

Mr. C. R. Markham has taken Dr. Shaw's place, and Mr. Carter Blake the place of Dr. Hunt. Mr. Thomas Wright was prevented by other engagements from attending the meeting this year. Mr. Crawfurd was present, fighting his battles with all comers, as of old. We venture to say that there is no man who attends the meetings of the Association who has had more fighting than this venerable member, and there is evidently no one who so thoroughly enjoys asking his friends "to tread on the tail of his coat." His opinions on all ethnological subjects are just as they were forty years ago, at which time Mr. Crawfurd, to his lasting honour, was one of the first to raise his voice against the stereotyped popular ideas which then existed respecting man's past history. We trust he may long be spared to attend the Association, and we feel sure that none will more heartily join in this wish than those who are most opposed to his scientific teaching and his antiprogressive ideas. We can scarcely ask of a hard-headed Scotchman of eighty years of age that he shall advance with the times; but we hope, for his own reputation, that Mr. Crawfurd will not, in his maturer years, help to arrest the cause of scientific truth, of which he has been so brave a champion during the last fifty years. We heartily coincide with Mr. Crawfurd's remarks on proposing a vote of thanks to the President, when he said, "Nature evidently intended Sir Roderick Murchison to be a President. He combined in the most happy proportions firmness and amenity, and always made the meetings over which he presided pleasant and profitable."

Before we conclude our report, we feel it our duty to express our earnest desire that Section E may long be spared that painful exhibition of personal animus which has, during the last few years, been introduced into the discussions of Section D. It will, perhaps, hardly be believed when we state the fact that, on a paper being read by Mr. Carter Blake, in Section D, on "Syndactyly in Man and Apes," a member of the Association was allowed to get up and make a long tirade against the writer of some anonymous article in the *Edinburgh Review* of April last. Not a word of discussion took place on Mr. Blake's paper, but the time of the Section was taken up by listening to Dr. Rolleston's grievances against this anonymous writer. We were glad, however, to hear the severe castigation which Mr. Blake administered to him, and which will, we trust, make him more cautious not to attack again in debate one who is evidently so much his superior.

Section E was much indebted to Professor Daniel Wilson, of

Toronto, for many excellent speeches. We are glad also to be able to find a full and original report of the excellent speech on Archæology made by Mr. George Tate, the well-known antiquary and geologist, and active Secretary of the Berwickshire Naturalists' Field Club. To the character of the speeches generally we will quote the words of the esteemed President, who said, "I have been a member of the Association from its foundation, and I must say that I never presided on any occasion on which there have been such numerous audiences, and so many admirable discussions. I may add, that I have never presided on any occasion on which I have seen so much good feeling exhibited, not only by persons around me on this platform, but by all those who have taken part in the proceedings." These were Sir R. Murchison's parting words to the Section, and we must now close our report. The Association has sustained a very severe loss in being deprived of the valuable services of Professor Phillips, who has from the commencement of the Association been its most energetic and able manager. A universal feeling of regret is felt throughout the Association at his loss. We hope he may long live to enjoy his well-earned popularity, and that his example will be the means of inducing his successors to follow in his disinterested and impartial footsteps. We believe that universal satisfaction is felt at the selection of Sir Charles Lyell as President for the ensuing year. Anthropologists have especial reason to be satisfied, for no one has of late years done more for the progress of Anthropological science than Sir Charles Lyell. We were glad to see him in Section E this year, and hope that for the future we shall see him far oftener. Everything bids fair to make the next meeting at Bath successful. We trust that during the time that will elapse before the meeting, Anthropologists will bestir themselves to bring all their forces together, and thus help to secure the formal recognition of Anthropological science by the Association. We understand that notice has been given by Dr. Hunt, that Section E shall for the future be devoted to "Geography, Ethnology, and Anthropology." A general rumour prevailed that there was to be a sub-section especially devoted to Anthropology. We think, however, that an increase of the number of sections is objectionable, and we see no necessity for such a division. As an independent journal, devoted to Anthropological science, we shall feel it our duty to advocate a union of Anthropology with the present Geographical and Ethnological section.